Essays in Financial Economics

by

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Abstract

This thesis examines three questions in Corporate Finance.

The first chapter investigates the effect of institutional ownership on the governance dynamics and behavior of firms. I exploit the exogenous change in equity index membership generated by the reconstitution of the Russell indices. Following reconstitution, I show that firms just included in the Russell1000 index have higher institutional ownership (IO) concentration than those just excluded - both a change in indexers and a change in active IO, suggesting a complementarity between these types of investors. Firms just included in the Russell1000 increase the performance sensitivity of their CEO's pay, have a higher likelihood of CEO turnover, and have lower capital expenditures. Overall, these results suggest a significant impact of institutional preferences on corporate behavior.

Chapter 2, joint with Antoinette Schoar, shows that CEOs' management styles and philosophy vary with the control rights of the founder and/or owning family, using a survey of over 800 CEOs in 22 emerging economies. CEOs of firms with greater family involvement have more hierarchical management, feel more accountable to stakeholders than they do to shareholders and see their role as maintaining the status quo rather than bringing change. In contrast, professional CEOs of non-family firms display a more textbook approach of shareholder-value-maximization. Between these types we find a continuum of leadership styles and philosophies that vary with how intensively family members are involved in management.

Chapter 3 examines whether and how companies benefit from campaign contributions. To obtain exogenous variation in such political connections, I use U.S. congressional elections

that were decided by less than 1% of votes in a RDD. Such close elections are akin to randomized assignment: Prior to the election, companies' political connections have similar expected values. My estimates suggest that companies connected to the winning candidate experience both a significant increase in long-term firm value and a positive short-term stock market reaction around the election date. I further document evidence supporting four channels through which political connections may enhance firm value: 1) allocation of procurement contracts, 2) reduced legislative and regulatory risks, 3) improved bank financing, and 4) improved access to lobbying.

Thesis Supervisor: Antoinette Schoar
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Chapter 1
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1 Introduction

The principal-agent problem between shareholders and managers has been a central concern in economics since the seminal work of Berle and Means (1932). This paper focuses on the effectiveness of shareholder monitoring as a mechanism to influence managerial behavior, and in particular, on monitoring by financial institutions such as index mutual funds and other institutional investors.

A large literature in corporate governance is built around the idea that institutional investors are relatively effective monitors of management both because of their sophistication and because their large size reduces the coordination and per-share costs of monitoring, and increases returns to governance (see, for example, Black, 1991; Gillan and Starks, 2000). However, it is difficult to identify the causal effect of institutional investors as monitors because of a well known endogeneity problem (Demsetz and Lehn, 1985) in which, among other things, institutions choose in which firms to invest so as to maximize returns. As a result they might choose to invest in firms they believe might benefit the most from additional monitoring, or conversely invest in firms they expect will have

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higher returns for other reasons. These selection issues imply observed cross-sectional correlations are likely to be a biased estimate of the impact of institutions as monitors.

We have even less evidence on the role - if any - that indexing and index funds play in corporate governance, despite the fact that the latter control a large and growing proportion of institutional assets under management: as of year end 2012, 24 percent of US institutional assets were in index funds and these funds are growing at a rate of 10 percent per year (Morningstar, 2013). Perhaps because of their volume-based, low margin business model, and passive stock selection approach, index funds are often believed to be unlikely to contribute in any way to monitoring of portfolio firms, and have been accused of being overly passive by activist investors (for example, Reuters, 2013: "U.S. activist investors gain from index funds' passivity").

Here I address the inference challenge by focusing on equity indices and by using exogenous variation due to the reconstitutions of the Russell 1000 index as an instrument for differences in index fund ownership. Firms are placed into the Russell equity indices for explicitly mechanical reasons, forcing index funds to hold their stock, breaking the link between firm and owner characteristics, and thereby obtaining clean identification of the causal effect of Index fund ownership on firm behavior. I use a regression discontinuity design (RDD) and find that firms whose index fund ownership rises as a result of index reconstitution do appear to be more heavily monitored, in contrast to the prevailing view of index funds which characterizes them as contributing nothing towards the monitoring of the firms they hold stakes in. I show that such firms raise the performance sensitivity of their CEO's pay without appearing to increase total pay, are more likely to have a new CEO within two years, have lower capital expenditures, and are less likely to engage in diversifying acquisitions in the two years following their inclusion in the index, in comparison to firms just excluded from the Russell 1000.

The natural experiment I consider compares firms near the threshold between the Russell 1000 and Russell 2000 equity indices. The Russell 1000 index (R1000 henceforth) contains the one thousand largest firms by market capitalization (i.e. firms ranked 1-1000), while the firms ranked 1001 to 3000 are placed in the Russell 2000 index (R2000 henceforth). On the last trading day in May, firms are ranked by a market capitalization measure and placed in each index for the entirety of the following year. If Firm A is ranked 1005th in May of year t-1 (i.e. in R2000) and then ranked 995th in May of year t, it will switch indices, moving from the R2000 to the R1000 in year t. Institutions that passively follow the R1000 are then obliged to buy the stock of firm A, and to sell the stock it replaces.

Importantly, in a narrow bandwidth around the index cutoff, whether a firm is placed in

the R1000 or R2000 is largely random, based as it is on small price shocks on the day(s) immediately preceding reconstitution, and also on the shocks to surrounding firms. This means that firms on one side of the cutoff are well suited to be controls for firms on the other side because the only dimension along which they differ systematically is a market capitalization ranking which is explicitly controlled for. I focus on the reconstitutions occurring in the years 2002-2006 for which I was able to obtain the proprietary Russell market capitalization measure that assigns firms to each side of the cutoff. The evidence indicates firms are unable to manipulate the ranking based on this variable: I perform the McCrary (2008) test to determine whether manipulation of market capitalization occurs and find no evidence of bunching around the cutoff. To further establish the validity of the RDD in this setting I show that firms within the bandwidth do not differ across the threshold in terms of a series of observable characteristics in the period before reconstitution, that placebo tests using alternative index cutoffs show no differential effects, and that estimates are robust to the inclusion of a variety of controls and specification choices.

I first document that index inclusion leads to significant changes in firms' institutional shareholder base: firms just assigned to the R1000 index (composed of larger firms) see their total institutional ownership (IO henceforth) jump up by approximately 10% of firm equity (equivalent to a jump of $150-$200 million), providing a clean natural experiment for the effects of institutional ownership on firm behavior. This change is driven by firms moving up from the R2000 into the R1000, and is asymmetrical: firms moving down do not experience a statistically significant reduction in institutional ownership over the following year.

Interestingly, not all the change in IO is attributable to changes in the holdings of passive index funds. Using Bushee's (2001) three category classification of investors based on portfolio diversification and turnover, as well as the Thomson Reuters 13F type code I show that institutions that are likely to be index funds increase their holdings by 4%-8% of firm equity, concentrated in firms moving up into the R1000. The remaining change in IO is attributable to funds that are not obliged to switch their portfolio in lock step with the index, although many ostensibly more active investors choose to stay very close to their benchmark indices, effectively "closet indexing" (Cremers and Petajisto, 2009), while others employ explicit quasi-indexing strategies. Importantly, I show that the holdings of the largest ten institutions in each firm also rises by around 6-11% of firm equity on average, and that this increase is not driven solely by index fund ownership. In particular, institutions with low portfolio diversification and low turnover - suggestive of an activist investor profile - take relatively larger positions in firms just included in the index, in comparison to those just excluded. In the regression discontinuity design employed in this

paper the increase in IO of non-indexers is also causally attributable to the instrument (index inclusion), suggesting a complementarity between index funds and other institutional owner types that appear more likely to exert an active monitoring role.

A natural question arises regarding which shareholders are displaced by the incoming institutional shareholders. Using a dataset of blockholder ownership collected by hand from proxy filings I show that total blockholdings do not differ across the threshold after reconstitution (either in terms of number or voting power), although there is weak evidence that blockholdings may increase in number. There are also no significant differences in insider (i.e. blockholder is an officer or director), outsider, institutional or non-institutional blockholdings, nor are there differences in the equity held by all officers and directors as a group. This implies that the increase in institutional ownership pushes out retail investors, the residual category. Given that retail investors are the group least likely to exert monitoring effort, it seems very likely that total monitoring rises.

I then turn to examining how ownership changes affects other governance dimensions within the firm. I first look at CEO compensation, which displays material differences in structure across the index cutoff, driven by firms moving up into the R1000. However, total CEO pay is not statistically higher for firms in the R1000 relative to the R2000 - the difference is instead in the structure of the pay package: option pay share is higher by 17% of total pay, which is offset by a salary pay share that is lower by 11% of total pay, and a restricted stock pay share that is 9% lower. This new pay structure, in which options are substituted for salary and restricted stock, is not unambiguously preferable for R1000 CEOs, and may in fact reduce their utility by increasing the amount of firm risk they bear without increasing total compensation. Additionally, the probability that a firm has a new CEO within two years of reconstitution is also 18% higher in firms just included in the R1000 relative to those just excluded, and this increase is concentrated among firms in the lowest quartile of stock returns in the first year in the index, suggestive of a stronger performance-turnover link in firms with higher institutional ownership.

The next governance dimension I examine is shareholder voting behavior at annual meetings to determine whether one of the central (and most observable) components of shareholder monitoring – voting at annual meetings – is affected by this change in IO. I find that the pass rate for management proposals is lower by approximately 3 percent for firms just in the R1000 relative to firms just included in the R2000, and the number of failed management proposals increases by 0.06 per meeting on average. This is a surprising result given management's structural control of shareholder meetings, reflected in the over 95 percent pass rate and the fact that management (almost) always wins close votes (Listokin, 2008).

While the number of both management and shareholder proposals is no different across the cutoff I also find that the number of failed shareholder proposals falls by 0.22 per meeting, on average, over the two years following reconstitution. In short, management appears to face a less pliable shareholder population when institutional ownership rises in the natural experiment considered here. While shareholder voting behavior is a natural place to seek effects of IO, institutions often exert the greatest monitoring influence through private meetings with management rather than through voting (see for example Carleton et al., 1998, and Becht et al., 2009), so these results are likely to understate the changes in governance taking place.

Finally, I look at firm outcomes, first examining the corporate accounting variables that the literature suggests may change in response to increased monitoring. The only robust result is that capital expenditure as a percent of assets is over 2 percent lower for firms just included in the R1000 over the two years following reconstitution. R&D as a percentage of sales - a variable the literature suggests is particularly sensitive to institutional ownership - is 8 percent higher, but not significant at conventional levels. Leverage, profitability, market-to-book, and payout (in terms of both repurchases and dividends) do not differ significantly across the threshold after reconstitution. However, acquisitions behavior is different across the threshold: firms just included in the R1000 make approximately 0.5 fewer diversifying acquisitions per year than firms just in the R2000.

The main RDD specification in this paper is locally linear regression, which controls for the RDD assignment variable - market capitalization rank - linearly. A potential concern is that what drives the observed differences between firms on either side of the threshold is not the change in institutional ownership but rather each firm's movement over the past year in terms of market capitalization - for example, firms that are rising quickly through the rankings, or falling rapidly - and the level of market capitalization might be poorly suited to capture this dynamic aspect of the firm's size. To allay this concern I also run the RD including as controls both the change in Russell's relative ranking and the change in the value of market capitalization over the past year.

Relatedly, it is possible that the randomization carried out by the RDD may be imperfect, as occasionally occurs with randomized experiments that fail to stratify their sample on enough dimensions. As a result, differences in covariates between firms on either side of the threshold might be driving the results. This is of particular concern for the results on CEO pay, which is known to covary strongly with size and, to a lesser extent, tenure and other firm and industry level variables. To assuage this concern I re-run the RD controlling for a battery of variables that conceivably co-move with the dependent variable. For

example, for the main CEO pay results I include controls for CEO age, CEO tenure, firm profits, sales, total assets, market value of the firm, sales growth, asset growth, market value growth, and number of employees. Coefficients and significance levels are largely unaffected, supporting effective randomization and validity of the RDD, as is also true when I include industry fixed effects.

The theoretical literature on managerial misconduct provides a variety of potential avenues for sub-optimal behavior from the perspective of the principal. For example, the manager may consume perks that do not enhance productivity (Jensen and Meckling, 1976); build "Empires" by focusing excessively on firm growth and acquisitions (Jensen, 1986) or "entrench" (Shleifer and Vishny, 1989); behave myopically in response to the signal extraction problem that the market faces in determining the firm's unobserved value (Holmstrom, 1982; Stein, 1989); or shirk and live the "quiet life" (Bertrand and Mullainathan, 2003).[1] These papers predict that if the main agency problems are empire building, entrenchment, and signal-jamming based myopia, increased monitoring should result in lower capital expenditures, greater R&D (which has low visibility and payoffs further in the future), and fewer acquisitions, especially diversifying ones. Alternatively, if the key agency problem is a desire for the quiet life and thus under-investment, then improved monitoring should, perhaps, be followed by greater capital expenditures and more acquisitions (albeit not necessarily diversifying ones) as management expends greater effort in expanding the firm.[2]

While the limited specificity of the theoretical predictions makes it difficult to argue that evidence clearly supports one class of models, the evidence in this paper is broadly consistent with the view that the main agency problems, at least as viewed by institutional investors, are related to over-investment rather than to the quiet life. Ferreira and Matos (2008) report that higher levels of independent and foreign institutional ownership are correlated with lower capital expenditures in cross sectional regressions in a large sample of non-US firms. However, they also highlight that institutional ownership is jointly determined with firm characteristics (and show that institutions have a preference for well-governed firms, among other characteristics), which means that the direction of causality remains an open question. Similarly, Baysinger et al. (1991) and Bushee (1998) show

[1] An additional stream of the literature posits managerial deviations from optimal behavior resulting from personality-based attributes of managers, such as acquisitions resulting from managerial hubris (Roll 1986), or overconfidence (Malmendier and Tate, 2008), and individual, fixed managerial styles (Bertrand and Schoar, 2003).

[2] It is theoretically possible that increased institutional ownership could exacerbate agency problems: Burkart et al. (1997) and Aghion and Tirole (1997), highlight the costs of monitoring in terms of foregone managerial initiative, and in Admati and Pfleiderer (2009) this occurs in some specifications, but requires the blockholder to sell its stake.

that institutional ownership is correlated with higher R&D expenditure, while Aghion et al. (2013) focus on R&D outcomes rather than inputs and instrument institutional ownership with S&P 500 inclusion, finding a positive effect on citation-weighted patenting. Two papers examine the effects of institutional owners of the acquirer on mergers and acquisitions activity. Chen et al. (2007) report that independent, long-term institutional blockholders are associated with better M&A performance, while Gaspar et al. (2005) find that long-horizon institutional shareholders are associated with higher bidder abnormal returns around merger announcements. Again however, the concern remains that the institutional shareholders whose portfolio firms have better acquisition outcomes may simply be better at identifying firms that make better acquisitions.

This paper provides new evidence on the link between Institutional Ownership concentration and firm growth strategies (capital expenditure, acquisitions, R&D), CEO pay, and CEO turnover by providing causal estimates not subject to concerns about reverse causality or simultaneous determination of institutional ownership and outcomes. It also may strengthen the evidence of active index fund monitoring provided by Matvos and Ostrovsky (2010), who show that, despite significant heterogeneity among such funds, S&P500 index funds can be very active monitors in terms of their voting behavior. Two contemporaneous papers have performed regression discontinuities using a similar quasi-experimental set up, but focusing on different aspects of the index inclusion. Chang et al. (2013) examine whether there is a price effect due to reconstitution in the month immediately following the reconstitution, and report a one month price effect, but no effect on liquidity or price volatility[3]. While they use a similar instrument they explicitly abstract from the governance effects which are considered here.

Crane et al. (2012) focuses on firms' payout policy. In contrast, my paper maps out the impact of index inclusion on firm governance, in particular on changes in the ownership structure of the firm, shareholder voting and CEO compensation and ultimately firm decisions such as M&A transactions, and investment policy. The other difference is that I use the proprietary market capitalization measure that that Russell Indexes uses to determine index assignment, and the results depend directly and materially on the use of this assignment variable. Crane et al. (2012) do not have access to such a measure, and instead use (a slightly transformed version of) the Russell index weights as the assignment variable. This identifies the wrong firms on either side of the discontinuity because after

[3] There exists a stream of literature that highlights the role of liquidity in governance (e.g. Bhide, 1993; Maug, 1998; Kahn and Winton, 1998; Faure-Grimaud and Gromb, 2004; Edmans, 2009; Admati and Pfleiderer, 2009; Edmans and Manso, 2011), but the lack of a liquidity difference across the threshold means I cannot extract testable predictions from them

Russell places firms in each index based on each firm's market capitalization ranking, weights are assigned based on a *different* market capitalization measure: the free float component of market capitalization. As such, index weight rankings differ from the true assignment variable - the market capitalization ranking that assigns firms to indices - and the firms it identifies as being on either side of the threshold differ materially in terms of their market capitalization and free float, violating the basic RD assumption of continuity across the threshold.[4] Intuitively, using Russell index weights to construct the assignment variable for a RDD compares the firms with the lowest free float (but not necessarily the lowest market capitalizations) in the Russell 1000 to the firms with both the highest market capitalizations and high free floats in the Russell 2000. This makes the firms on either side of the index cutoff inappropriate counterfactuals for each other, and as a result the use of the index weight ranking as the assignment variable (instead of the market capitalization ranking) for a RDD generates results with very different significance levels, coefficient magnitudes and even opposite signs in some cases.

This paper also contributes to the relatively small literature on CEO pay and institutional ownership. CEO compensation is a natural focus of institutional interest: a survey by McCahery et al. (2011) reports that institutions believe CEO compensation and ownership to be the two most important governance mechanisms. Hartzell and Starks (2003), and Almazan et al. (2005) report a positive relationship between institutional investor concentration and the performance sensitivity of CEO pay, as do Clay (2000) and Schmidt (2013), while Morse et al. (2011) provide evidence that institutions may reduce powerful CEOs' ability to rig pay composition to their advantage.

The natural experiment provided by the Russell Index reconstitution provides intriguing evidence of apparent complementarity in ownership between index funds and other institutional investor types. It is possible that the presence of index funds reduces coordination difficulties for non-blockholder institutional investors or activist investors. Equally, index funds might be complementary to other blockholders, augmenting their ability to align managers' interests with those of shareholders. More generally, it is unclear whether the central governance channel at work in this natural experiment is the large blockholder "voice" channel (Shleifer and Vishny, 1986; Burkart et al., 1997; Maug, 1998; Kahn and Winton, 1998) given index funds' large size and inability to sell their positions unless the index changes, or the "exit" channel (Parrino et al. 2003; Edmans, 2009; Admati and Pfleiderer, 2009; Edmans and Manso, 2011) because the non-index fund institutional ownership that appears to move with index fund ownership is capable of relatively rapid

[4] Chang et al. (2013) also highlight the unsuitability of using the rankings based on index weights as a RD assignment variable

exit. Both channels may be at work simultaneously, as might occur if funds within the same families coordinate to increase their governance impact (see for example Morgan et al., 2011) both in terms of voting and in terms of exercising governance via exit (or the threat of exit) by non-index fund members of the fund family. Finally, whether the changes documented here are value enhancing remains an open question, but the evidence presented here suggests that there is no value effect on average over the two years following index inclusion.

The structure of the paper is as follows. Section 2 describes the data. Section 3 describes the details of the Russell Index reconstitution, discusses the methodology, and provides evidence for the validity of the RDD. Section 4 reports the results, and section 5 concludes.

2 Data and Summary Statistics

I obtain Russell index constituents, index weights, and proprietary free float and market capitalization measures from Russell Indexes for 2002-2012 (Russell's proprietary measure of market capitalization is available only from 2002). Institutional ownership data is extracted from Thomson-Reuters Institutional Holdings (13F), which provides the equity positions every quarter end of all institutions that exercise investment discretion over at least USD $100 million. Thus, institutions managing less than this amount are not in my data. I match this data by cusip and quarter to Quarterly CRSP-Compustat Merged data to obtain quarterly shares outstanding, because I find much of the shares outstanding data in the 13F data to be stale. I also remove stale 13F observations that are duplicate observations by manager number, cusip, fdate and rdate, keeping the oldest observation. I use Bushee's (2001) permanent three category classification of institutional investors as (i) "Quasi Indexers" (low turnover, high diversification), (ii) "Transient Investors" (high turnover, high diversification) and (iii) "Focused Investors" (low turnover, low diversification, which he calls "Dedicated"), available on his webpage to classify investors into types. To generate a proxy for index funds (which I term "Indexers") I generate an indicator equal to one if the firm is classed as a "Quasi-Indexers" and if the Thomson Reuters 13F type code is that of an Investment Company or an Independent Investment Advisor (I use Bushee's extension of this, which does not distinguish between these types accurately). While this is a noisy identifier of index funds it does capture all the largest index funds managers that cover the Russell 1000, such as Dimensional Fund Advisors and Vanguard. I match the resulting dataset to the Russell data by cusip.

Table 1 reports summary statistics for the 13F data at the baseline (i.e. in the period(s) before index reconstitution), for a bandwidth of 100 firms each year, presented separately in the R1000 and the R2000. The final column presents the p value of a t test for differences in means across the R1000 and R2000 groups. No differences are significant except for market capitalization, which is expected given that it is the variable underlying the assignment variable of the RDD.

Blockholder information is collected by hand from annual proxy filings (Schedule 14A and 14C) available from the SEC's EDGAR database. I broadly follow the method described in Dlugosz et al. (2006) and Clifford and Lindsey (2011), although I also collect the total votes held by all officers and directors as a group, even if it is below the 5% threshold (it is always reported). I further use Schedule 13D and 13C information to determine whether shareholders have filed as 13D (investors with active intent towards the firm). Each proxy filing was carefully checked for double counting of holdings, which occurs frequently. Detailed decision rules for how the data was recorded are available from the author. Firms were randomly selected for the sample from within the bandwidth of 100 firms on either side of the threshold. Summary statistics obtained from Proxy filings in the year prior to reconstitution are in **Table 2**. It is worth noting that 65% of the annual meetings in my sample occur in April and May, the months immediately preceding reconstitution, and this is the point at which I obtain the baseline information on blockholdings. If blockholders strategically anticipate index inclusion to some degree, even if imperfectly, then this will generate some differences across the threshold at the baseline because of the short period of time between my baseline measure and the reconstitution event. Nonetheless, there are very few significant differences (although several are almost significant at the 10% level) and they do not appear to be economically large or significant. The average number of votes controlled by institutional shareholders appears to differ significantly in the table. However, on assignment the main specification of the RD with this baseline value as the dependent variable (i.e. after controlling for the assignment variable: market capitalization rank), the difference between firms in the R1000 and firms in the R2000 is not significant.

CEO compensation data is drawn from Execucomp. I identify CEOs using the CEOANN variable; for firms for which this does not identify the CEO I follow Landier et al. (2013) and use the information on the date the CEO took up the position and left the position (variables BECAMECEO and LEFTOFC) to identify the CEO, if possible. I drop firms whose CEO I cannot identify using the procedure just described, CEOs of firms with more than one CEO (fewer than ten), and CEOs recorded as becoming CEOs more than a year after the CEOANN variable reports them as being CEO of the firm "for all or most" of the fiscal year. I calculate option deltas using the method in Core and Guay (2002). To

identify whether the CEO has changed in the years subsequent to index inclusion I use the information in the LEFTOFC (left office of CEO) variable, and remove from this measure CEOs that are reported in the reason variable as having left office because they died. Details regarding the construction of specific variables are provided in the legend of the summary statistics table. **Table 3** presents summary statistics for a variety of Execucomp variables in the baseline period: there are no statistically significant differences except for the share of pay in the form of perks (a 2% difference) and total pay (only significant at the 10% level and to be expected if pay covaries with firm size).

I match the Russell data to the CRSP-Compustat Merged database by historical cusip and year, and hand match firms with stale cusips in the Russell data.[5] I obtain month-end stock prices from CRSP, and quarterly shares outstanding from the Quarterly Compustat database. I winsorize accounting variables that are not naturally top or bottom censored at the 2% level yearly to reduce the influence of outliers - this does not affect the results. Summary statistics for the Compustat-CRSP merged data in the period immediately preceding reconstitution are in **Table 4**. Repurchases as a percentage of outstanding equity appear significantly different across the index cutoff. However, they are not significant in a RDD run with the baseline values as the dependent variable (i.e. controlling for the assignment variable, market capitalization rank).

Acquisitions data is from the SDC Platinum database and consists initially of all mergers and acquisitions with US based acquirers or US based ultimate parents of the acquirers. Following the literature (e.g. Netter et al., 2011) I drop deals with missing announcement dates, deals between firms and their parents, deals that are not completed, deals where the acquirer already held a majority stake before announcing the deal, and deals where final ownership is below 95 percent or where final ownership cannot be inferred from the SDC data. I also drop deals occurring in July and August, as they are very likely to have been in process when the index reconstitution occurred due to the lead times required of a large transaction. For variables that use deal values I use only the sample that contains such values, and I drop deals with values of below USD $1 million or below 1 percent of acquirer assets. **Table 5** presents summary statistics.

Finally, Voting data comes from RiskMetrics. I drop proposals with missing information

[5] Because Compustat (and Execucomp) data is reported at fiscal year end, and Index reconstitutions occur near the end of June, using the data of firms reporting in the months immediately after the June reconstitution would lead to some firms only having received "treatment" (i.e. inclusion in the Russell 1000) for a few months. Moreover, many accounting variables are slow moving, and often determined at a yearly frequency (such as investment plans). I follow Cuñat et al. (2012) and require firms to have been treated for more than 6 months: firms that report July-December have their first year's values replaced with those of the following year.

for pass/fail or vote totals, and keep only Annual Meetings. I match this data to my Russell-CRSP merged data by ticker, month and year. I then match remaining unmatched observations by ticker and year, and hand check all matches to ensure correct matching, dropping mismatched observations. I hand match the remainder of the observations as far as possible. Finally, for firms with more than one annual meeting in a Russell year (that runs from1 July in year t to 30 June in year t+1) I keep the annual meeting that is latest in the year, to ensure the firm has been treated for as long as possible. For the remaining observations and out of an abundance of caution I drop annual meetings that occur in July, August and September, because shareholders of record are likely to be the owners from before index reconstitution rather than the owners that result from the natural experiment considered here. **Table 6** presents summary statistics for these variables within a 100 firm bandwidth, separated by index.

3 Russell Index Construction

The construction of the R1000 and R2000 indexes that provide the natural experiment for this paper depends on firms' equity prices on a single day every year: the last trading day in May. At the market close of that day all firms are ranked by Russell according to their measure of market capitalization. According to Russell, this alone determines index membership. However, Russell considers their market capitalization measure, and the ranking it generates, to be proprietary information which it is unwilling to share.

How then did Russell Indexes compute the market capitalization of index constituents in my sample period? Firstly, Russell only included firms with headquarters in the US, and whose stocks trade at or above $1.00 on May 31. In addition, Russell must have had access to appropriate "documentation" regarding the company, and used FT Interactive data as the primary source for prices and total shares outstanding, although it also used information on share changes reported to the SEC and other unspecified sources "in cases of missing or questionable data" (Russell, 2004). Market capitalization is calculated by multiplying the total number of all outstanding common shares of all classes (but excluding non-common shares such as preferred stock) by the price of the "primary trading vehicle" only, as determined by Russell using a rule based on the relative trading volume, price and free float of the different types of equity securities outstanding.

Once index membership is determined by the firm's rank in terms of Russell market capitalization, Russell then determines index weights by reducing each firm's market capitalization by the number of shares not held to be freely floating according to their proprietary

measure (holdings by institutions are not considered to reduce a firm's free float). This adjustment takes place *within* each index. Thus, firms in the Russell 1000 ranked around 800 but with a low free float could have a weight equivalent to that of a firm ranked 900 or lower by market capitalization. This means that firms near the bottom of the R1000 will include firms with much higher market capitalization – but low free float – than firms near the top of the R2000, while firms at the top of the R2000 will have both the highest market capitalizations in the R2000, and among the highest free floats. As a result, a regression discontinuity using *firm rank by index weights* as the assignment variable will generate spurious results because the firms on either side of the cutoff are substantially different in terms of – at a minimum – market capitalization and free float as determined by Russell, and as such are invalid controls for each other.

Finally, actual reconstitution, as opposed to the determination of index membership, occurs on the last Friday in June. However, changes in the constituents are announced before then, and "subject to change if any corporate activity occurs or if any new information is received prior to release" (Russell, 2004). The fact that index weight ranks are of no use for a RDD leaves the researcher with two alternatives: attempt to reconstruct the information used by Russell at the time, or to obtain the information directly from Russell themselves. I was unable to reconstruct Russell's proprietary market capitalization ranking adequately using CRSP and Compustat data, which is relatively unsurprising given that Russell appears to purposely make it hard to predict which firms will be included at the margin, so as to protect index customers from trading that seeks to exploit their relatively inelastic demand on the index reconstitution date.[6]

However, Russell Indexes consented to provide me with a slightly noisy version of their market capitalization variable from 2002 onwards – they did not retain the information before then. Unfortunately, Russell Indexes changed their index construction method starting in 2007, and implemented a banding procedure based on the market capitalization of the 1000th largest firm. This procedure was designed to eliminate index changes for firms whose market capitalization is little different to that of the cutoff firm, which is precisely the variation I exploit to identify my estimates. As a result the number of firms moving from the R2000 to the R1000 in 2007 drops to around 20, down from 90 the previous year, and market capitalization is no longer smooth across the 1000 rank cutoff. This change greatly reduces the power of the natural experiment, so for this reason my sample ends in 2006.

[6] For the purposes of the natural experiment considered here, the non-public nature of Russell's measure, and the difficulty of replicating it accurately, reduces the ability of firms to manipulate their relative ranking precisely, bolstering the validity of the RDD

The proprietary measure of market capitalization provided to me by Russell consists of the previous year's portfolio's market capitalization, updated to May 31 of the current year. This is a good approximation of the final variable, but not a perfect fit because (i) it does not include all firms that list during the year (ii) the primary equity vehicle which determines price may have changed over the year (iii) the firm may no longer be eligible to be included in next year's index, (iv) new information may have become available during June, or existing information may have been deemed questionable.

Figure 1 displays a plot of the underlying market capitalization variable relative to the Russell rank, and the smoothness across the threshold supports the validity of the RD design. However, because the market capitalization variable I obtain from Russell Indexes is not the exact variable used for assignment, but one measured with what is likely random error, I implement a Fuzzy RD design, instrumenting index inclusion with an indicator that equals one when firm market capitalization rank is 1000 or below. The first stage of this fuzzy RD generates an adjusted R2 of 0.83 and a coefficient on the instrument of 0.72 (t-stat of 14.9) – a coefficient of 1 would indicate a sharp RD – and is thus a strong instrument, allaying potential concerns regarding weak instruments.

4 Methodology

I estimate the causal effect of institutional ownership on firm outcomes using a regression discontinuity design (RDD). The intuition behind the RDD is as follows. Firms in a narrow bandwidth around the R1000 – R2000 boundary are quasi-randomly assigned to one of the two indices, and index tracking behavior by institutional owners generates plausibly exogenous variation in total institutional ownership across the boundary in response to this assignment.

Importantly, in a narrow bandwidth around the index cutoff, whether a firm is placed in the R1000 or R2000 is largely random, based on small price shocks on the day(s) immediately preceding reconstitution, and also depends on the shocks to surrounding firms. This means that firms on one side of the cutoff are well suited to be controls for firms on the other side, as the only dimension along which they differ systematically is the Russell market capitalization ranking. Because the indices are associated with different populations of institutional investors with different quantities of assets under management, this generates a difference in total institutional ownership across the index boundary. In turn this may have effects on firm outcome variables subject to monitoring by shareholders.

The difference in these outcome variables between firms in the R1000 and R2000 is what the RDD measures.

The RDD's suitability for causal inference derives from the relatively mild assumptions it requires. RDDs rely on a key assumption of imprecise control (Lee and Lemieux, 2010): that companies cannot precisely predict or control their Russell market capitalization rank, and thus cannot choose to be in the R1000 or R2000. This assumption implies that, in the absence of the index reconstitution the outcomes of firms just below the cutoff and those above would have been similar, so the only reason that the actual outcomes are different is that after the index reconstitution some firms are in the R1000 and thus will have shareholders that are benchmarked to the R1000, while other firms will be in the R2000 and have the corresponding shareholders.[7]

The assumption of imprecise control over firms' Russell market capitalization rank implies firms are randomly assigned to treatment, here defined as membership in the Russell 1000. Unlike an instrument variable's exclusion restriction, this assumption has three main testable implications: 1) Observed pre-determined characteristics should be identically distributed on either side of the index cutoff; 2) the density of firms on either side of the threshold should be the same; 3) RDD estimates estimate of should not vary greatly when we include baseline covariates, as these are not required for consistent estimation of the treatment effect. I provide evidence for all three in the following section.

I implement the fuzzy RDD following the standard procedure using two stage least squares. I pool all firm-year observations, including year fixed effects, and estimate the following regression:

$$Idx_{it} = \gamma_{0r} + \gamma_{1r} Mktcap\ rank_{it} + T_{it}[\gamma_{0l} + \gamma_{1l} Mktcap\ rank_{it}] + \delta_t + \epsilon_{it}$$

$$Y_{it} = \alpha_{0r} + \alpha_{1r} Mktcap\ rank_{it} + Idx_{it}[\alpha_{0l} + \alpha_{1l} Mktcap\ rank_{it}] + \delta_t + u_{it}$$

Where i indexes firms, and t indexes years. Equation 1 is the first stage: Idx is an indicator equal to one if the firm is placed in the R1000 index after reconstitution; T is an indicator

[7] Fuzzy RDD also requires two further assumptions (see Hahn et al., 2001). Firstly, monotonicity (i.e. crossing the cutoff cannot cause some units to take up the treatment and others to reject it) which is clearly satisfied by the Russell index procedure. Secondly, excludability (i.e. crossing the cutoff cannot impact the outcome variable other than through impacting receipt of treatment). This is likely true for most corporate finance variables in the narrow range of values of the assignment variable considered by the main specification used in this paper. However, it may pose a problem, at least formally, for dependent variables such as leverage, security issuance, and market-to-book. These are not the focus of this paper.

equal to one if the firm's market capitalization exceeds that of the 1000th ranked firm, and is used as an instrument for Idx. Mktcap rank is the firm's Russell market capitalization rank – the assignment variable – minus 1000 to center the data on the cutoff. Subscripts r and l indicate coefficients estimated on data exclusively to the right and to the left respectively: the regressions are estimated by triangle kernel weighted OLS separately on each side of the cutoff. Equation 2 is the second stage, and delta is a vector of year fixed effects. The above specification is estimated on a relatively small bandwidth of 100 observations to either side of the cutoff, and corresponds to local linear regression fuzzy RD.

Using a wider bandwidth provides additional statistical power at the cost of introducing greater bias because the RDD's randomization result is local: as one moves away from the cutoff it becomes increasingly less true that the firms on either side are similar ex ante. An alternative is to use a wider bandwidth and to control for increasing heterogeneity across the boundary using a flexible polynomial function. However, in their benchmarking of the RDD against experimental data, Black et al. (2007) report that local linear regressions have lower bias and less specification-sensitivity than polynomial regressions. Accordingly, the local linear regression above is the preferred specification throughout. The bandwidth choice of 100 firms on either side was chosen for simplicity and comparability of the sample across estimates, but in the robustness section of the paper I show estimates are generally robust to both smaller and larger bandwidths (as well as polynomial functions with very large bandwidths). Moreover, I provide an estimate using Imbens and Kalyanaraman's (2012) optimal bandwidth, constrained to a maximum of 200, and show that in general the algorithm-selected bandwidth is close to 100.

The above specification includes year fixed effects. While fixed effects (of any type) are not required for consistent inference in the RDD, they mitigate concerns that certain years may be different from other. In robustness checks, I show that I obtain similar results if I include SIC division fixed effects to capture unobserved heterogeneity at the industry level, or if I exclude year fixed effects. I also include a variety of covariates, including the change in the market capitalization and in the Russell market capitalization rank of firms over the preceding year. It is worth recalling, however, that a valid RDD with a local linear specification and a small bandwidth – my main specification – does not require the inclusion of covariates beyond the assignment variable for identification or consistency, and is not subject to omitted variable biases. To account for any potential within-firm dependence over time, I cluster standard errors by firm, although results are robust, and generally more statistically significant, when Huber–White standard errors are used, as is standard for pooled RDDs.

Lastly, I estimate the local linear regression above using a triangular kernel. This kernel has been shown to be optimal in estimating such regressions at boundaries (Fan and Gijbels, 1996). The triangular kernel weights observations nearer the threshold more than those further away – intuitively, the triangular kernel gives more weight to firms whose index placement is more likely to be random. Results are robust to using a uniform kernel in narrow bandwidths.

4.1 Tests for Quasi-Randomized Assignment

My identification strategy relies on random assignment to the "treatment" (i.e., being placed in the R1000). As mentioned in Section 3.2, this assumption has testable implications, akin to the tests of effective randomization in experimental data.

The first testable implication is that the distribution of the assignment variable should not exhibit any bunching around the discontinuity, as this constitutes prima facie evidence that firms can manipulate their value of the assignment variable, suggesting a violation of the key assumption of imprecise control. While the assignment variable in this case is a ranking, and so by construction is uniformly distributed over its range, I perform the McCrary (2008) test for discontinuities of the density of the underlying variable: Russell market capitalization. This test is run year by year as the distribution of market capitalizations shifts up every year in my sample. The absolute value of the average yearly t statistic for the McCrary test of discontinuity in the density of the log Russell market capitalization for a bandwidth of 500 firms is 0.59; for a bandwidth of 100 firms it is 0.81; none are statistically significantly different from zero at any conventional level, and the coefficients for the discontinuity fluctuate between positive and negative. In short, I am unable to reject the null of continuity of the density function at the threshold, suggesting firms are not manipulating their market capitalizations to ensure they are on a specific side of the Russell Index threshold.

The second testable implication is that firms to the left and to the right of the cutoff should be similar on the basis of ex ante characteristics. If they differ, then the treatment would not appear to be randomized and we would infer that companies are able to predict the election outcome and sort themselves accordingly. In the Data section I present summary statistics for a series of covariates in the baseline period immediately preceding reconstitution. The difference-in-means test provided in the last column of each summary statistics table confirms that the average difference in each characteristic in the bandwidth is statistically insignificant. In untabulated regressions, I further estimate the RDD with

each baseline covariate as the dependent variable, and confirm the result that there do not appear to be systematic differences across the threshold before reconstitution, aside from those that are to be expected as a result of how the indices are constructed.

A third testable implication of random assignment to treatment is the relative invariability of estimates to the inclusion of baseline values of covariates and fixed effects. If the RD is valid, covariates beyond the assignment variable (and functions thereof) are not required for identification or consistency, and serve simply to reduce sampling variability, especially with a local linear specification in a narrow bandwidth. Thus they should not change the value of the coefficient materially on average, although some fixed effects could reduce the available variation to such an extent that little remains for estimation. In the Robustness section I show this is the case for the main results of the paper by including a battery of control variables and industry fixed effects[8].

5 Results

Tables 7.1 to 7.3 document that index inclusion leads to significant changes in firms' institutional shareholder base. The coefficients displayed are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000.

In Table 7.1 we see that firms assigned to the Russell 1000 index (composed of larger firms) see their institutional ownership (IO) jump up by approximately 10% of firm equity (equivalent to between \$150-\$200 million) at the end of June, immediately following reconstitution. This provides evidence that there is clear natural experiment for the effects of institutional ownership on firm behavior associated with this index reconstitution threshold. This change is driven by firms moving up from the R2000 into the R1000, as is

[8] In unreported results I re-estimate the RDD for a number of placebo thresholds instead of the 1000 rank; no discrete jumps are observed.

clear from the second panel, and is asymmetrical: firms moving down do not experience a statistically significant reduction in institutional ownership, as can be seen in the third panel, suggesting that offsetting movements from non-index funds dampen (amplify) the effect on institutional ownership when a firm is just excluded (included) from the index. In unreported results I show that the number of institutions owning the firms' equity does not differ across the cutoff.

Table 7.1 also shows that the ownership stake of the top 10 institutions by ownership in the sample firm also rises discontinuously on index inclusion. In particular, for the "up sample" (i.e. the second panel, composed of firms moving up and firms staying in the R2000) the increase is of approximately 7-10% of firm equity. This suggests a potential complementarity between index funds and other institutional owner types more likely to monitor, because the largest institutional owners have the greatest incentive to perform active monitoring. In the RD graphs section I present graphs of the RD estimate, along with a scatterplot of the data averaged over 10 or 15 rank positions to give an idea of the shape of the underlying data. The X axis has the assignment variable, while the Y axis presents the dependent variable. The estimated coefficients correspond to the difference between the estimated functions to the left and to the right at the index cutoff.

Table 7.2 presents the RD estimates for the top 10 institutional owners identified as "Indexers." These are institutions classified as Quasi-Indexers by Bushee's algorithm (i.e. high diversification and low turnover) and that are classified as an "Investment Company" or "Independent Investment Advisors" using the type code variable from Thomson Reuters' 13F data, as extended on Brian Bushee's website. While this is a noisy measure of true indexing if closet indexing is widespread, it captures the pure index funds (which tend to be very large, and hence in the top ten institutionals by category), as well as supposedly active mutual funds that actually have low portfolio turnover. The pattern of ownership change across the threshold displayed here is similar to that of both total IO and IO of the top 10 institutions in Table 7.1, suggesting that quasi-index funds are, at a minimum, partially driving the ownership change. Moreover, the noisiness of the measure likely explains why we do not see a statistically significant reduction in indexer IO for the down sample. For completeness I also include the RD estimated of ownership of the top 10 quasi-index institutional owners, which also display a similar pattern. Table 7.3 displays the RD estimates for the top 10 institutions in Bushee's other two institutional owner categories: Focused Investors (low diversification, low turnover) and Transient Investors (high diversification, high turnover). Focused investors (as well as transient investors) appear to increase in both the overall sample and in the up sample, suggesting that investors that are likely active monitors comove with index funds into the firm's shareholder base.

A potential concern might be that institutions, by increasing their ownership after index reconstitution, displace large owners or blockholders that were active monitors of the firm, producing an overall reduction in monitoring, and changing the interpretation of the results. Using a dataset of blockholder ownership collected by hand from proxy filings **Table 8** shows that there appear to be is no difference in total blockholdings after reconstitution across the threshold, nor are there significant differences in insider (i.e. blockholder is an officer or director), outsider, institutional or non-institutional blockholdings. There is some evidence that the number of blockholdings is 0.77 higher on average in the R1000 relative to the R2000 after reconstitution (and within the bandwidth), and in general the coefficients on blockholdings, both in terms of numbers of blocks and of votes controlled indicating that, if anything, there might be an increase in blockholder voting power or numbers on entering the R1000, although this increase is not statistically significant. The fact that blockholdings are not reduced (and may even increase) after reconstitution implies that the increase in institutional ownership pushes out retail investors, the residual category. Given that retail investors are the group least likely to exert monitoring effort, it seems likely that total monitoring rises when such investors are replaced by institutions, particularly when much of the increase in IO is concentrated in the top 5 institutional owners, as is shown in Tables 7.2 and 7.3.

Table 9 displays results for CEO pay in the year following reconstitution. Compensation also displays material differences across the threshold, driven by firms moving up into the R1000. For such firms CEOs' option pay share is higher by 17% of total pay, which is offset by a salary pay share that is 11% lower, a restricted stock pay share that is 9% lower, and a long term incentive pay share that is 9% lower. However, this different weighting of the components of CEO pay does not result in significantly higher total pay overall for CEOs of firms in the R1000 relative to the R2000. Moreover, it can be argued that the new composition of CEO pay is unfavorable to the CEO, which if true would suggest active monitoring and reduced CEO power when faced with an altered shareholder population. The first two columns show the probability that a firm has a new CEO within one and two years of reconstitution. There is no statistically significant difference in the one year probability, but the two year probability is 18% higher for firms in the R1000 relative to firms in the R2000, suggestive of a potentially stronger performance-turnover link in firms with higher institutional ownership.

Turning to firm outcomes in **Table 10**, I examine a variety of corporate accounting variables that the literature has argued may change in response to increased monitoring. The central result is that capital expenditure as a percent of assets is over 2 percent lower for firms just included in the R1000 over the two years following reconstitution. R&D as a

percentage of sales is 13 percent higher (23 percent for firms moving up into the R1000) but, perhaps because of the relatively few firms reporting non-missing values for this variable, this is not significant at conventional levels: the p value is 0.11 (0.14). Leverage measures, profitability, market-to-book, and payout (both in terms of repurchases and of dividends) do not differ significantly across the threshold.

Acquisitions behavior is different across the threshold, as is evident in **Table 11**. Firms just included in the R1000 make 0.5 fewer cash deals than firms just in the R2000, and these tend to be of mainly private targets. Moreover, firms just in the R1000 make approximately 0.5 fewer diversifying acquisitions, and fewer large diversifying acquisitions in relation to the acquirer's size.

To make some progress on the channels through which these institutional investors affect governance, I examine shareholder voting behavior at annual meetings in **Table 12** to determine whether one of the central and observable components of shareholder monitoring – voting at annual meetings – is affected by this change to IO. The pass rate for management proposals is lower by approximately 3 percent for firms just in the R1000 relative to firms just included in the R2000, and the number of failed management proposals increases by 0.06 per meeting on average.

This is a striking result given management's structural control of shareholder meetings, reflected in the over 95 percent pass rate and the fact that management almost always wins close votes (Listokin, 2008). While the number of both management and shareholder proposals is no different across the cutoff the number of failed shareholder proposals falls by 0.22 per meeting, on average, over the two years following reconstitution. In short, management seems to face a less pliable shareholder population when institutional ownership rises. While this is a natural place to seek effects of IO, institutions often exert the greatest monitoring influence through private meetings with management rather than through voting (see for example Carleton, Nelson, and Weisbach, 1998, and Becht, Franks, Mayer, and Rossi, 2009), so these results might understate the actual changes in governance taking place.

6 Robustness

To evaluate the robustness of the main results, in **Tables 13 and 14** I present a variety of specifications for the main results on CEO Pay and Accounting variables. The first column

is the main specification from the preceding tables. Columns 2 to 4 are described in detail below. Column 5 removes year fixed effects to evaluate the sensitivity of results to their inclusion. Column 6 winsorizes the data at 10% for both tails to evaluate the dependence of the results on outliers. Columns 7 and 8 present smaller and larger bandwidths for our main local linear specification. Column 9 uses a large bandwidth and a flexible polynomial - an alternative specification that is often used when power is limited. Finally, column 10 presents the main local linear specification using the Imbens and Kalyanaraman (2012) optimal bandwidth. Results appear robust to these permutations of the sample and or specification.

As noted earlier, my main RDD specification is locally linear regression, which controls for the assignment variable linearly. However, one might be concerned that this specification does not adequately capture the difference between firms on either side of the threshold, because it is a level variable that does not incorporate each firm's movement over the past year in terms of market capitalization – for example, firms that are rising quickly through the rankings, or falling rapidly. One could be concerned that it is this dynamic aspect of the firm's market capitalization that drives the results rather than the change in institutional ownership. To allay this concern I run the RD for the main results including as controls both the change in Russell's relative ranking over the past year, and the change in the value of market capitalization. Coefficients and significance levels are unaffected.

Relatedly, it is possible that the randomization carried out by the RDD may be imperfect, as occasionally occurs with randomized experiments that fail to stratify their sample on enough dimensions. As a result, differences in covariates between firms on either side of the threshold might be driving the results. This is of particular concern for the results on CEO pay, which is known to covary strongly with size and, to a lesser extent, tenure and other firm and industry level variables. To assuage this concern I re-run the RD for the main results controlling for a battery of variables that conceivably co-move with the dependent variable. For example, in Table 12 for the main CEO pay results I include controls for CEO age, CEO tenure, firm profits, sales, total assets, market value of the firm, sales growth, asset growth, market value growth, and number of employees. Coefficients and significance levels are unaffected, supporting effective randomization and validity of the RDD, as is also true when I include industry fixed effects.

7 Conclusion

I use exogenous variation in institutional ownership to examine the dimensions through which institutional investors in general and index investors in particular affect corporate behavior. Institutions are by far the largest group in the US public shareholder base; index funds control approximately a quarter of US institutional assets under management, while the volume of assets managed by quasi-indexers makes passive owners a majority of the shareholder population of many public firms. Institutions - at least of some types - are widely believed to be active monitors, and they are better equipped to monitor the firms they invest in than the retail investors they have progressively replaced. The degree to which this monitoring occurs via voice – engagement with the firm either privately or by voting or via exit from the position is still an open question, but the existing evidence suggests both are important.

This potential for such different types of monitoring makes empirical inference with regard to the effects of institutions on firms difficult, as does the fact that institutions choose which firms to invest in, potentially matching to certain types of firms. The type of the firm in turn may be correlated with the outcome variables, rendering invalid any conclusions about the independent effect of IO on firm outcomes. By focusing on information-free changes to an equity benchmark I address these problems. Index funds have no discretion to choose which firms to invest in and no ability to exit the shareholder base instead of exercising voice. This allows clean estimation of the effects of index funds on firm governance and outcomes. I also show evidence of an apparent complementarity between explicit index funds and other institutional investor types, as the latter accompany index funds into firms that are just included in the index relative to firms that are just excluded.

The theoretical literature is not very specific about, nor in agreement regarding the predicted direction of all of the effects considered here. Empire building and entrenchment theories, for example, would suggest that increased monitoring would result in reduced capital expenditures and acquisitions, as I report in this paper, in contrast to the predictions of the quiet life theory. Increased sensitivity of CEO turnover to performance could also induce less risk taking on the part of the CEO, leading to potentially reduced (diversifying) acquisition activity, as I find in my data. Changes to CEOs' compensation are likely to have important effects on firm risk taking, and a stream of literature exists that focuses on this relationship (for example see Gormley et al., 2013). However, given the yearly frequency of index reconstitution the horizon that I am able to examine is too short for the flow effects of changes in compensation structure to outweigh the stock of accumulated incentives.

This paper shows that institutions that closely follow an equity index appear to have large causal effects. Given the increasing popularity of indexing, exploring these effects further, especially their value implications, is an important question for future research.

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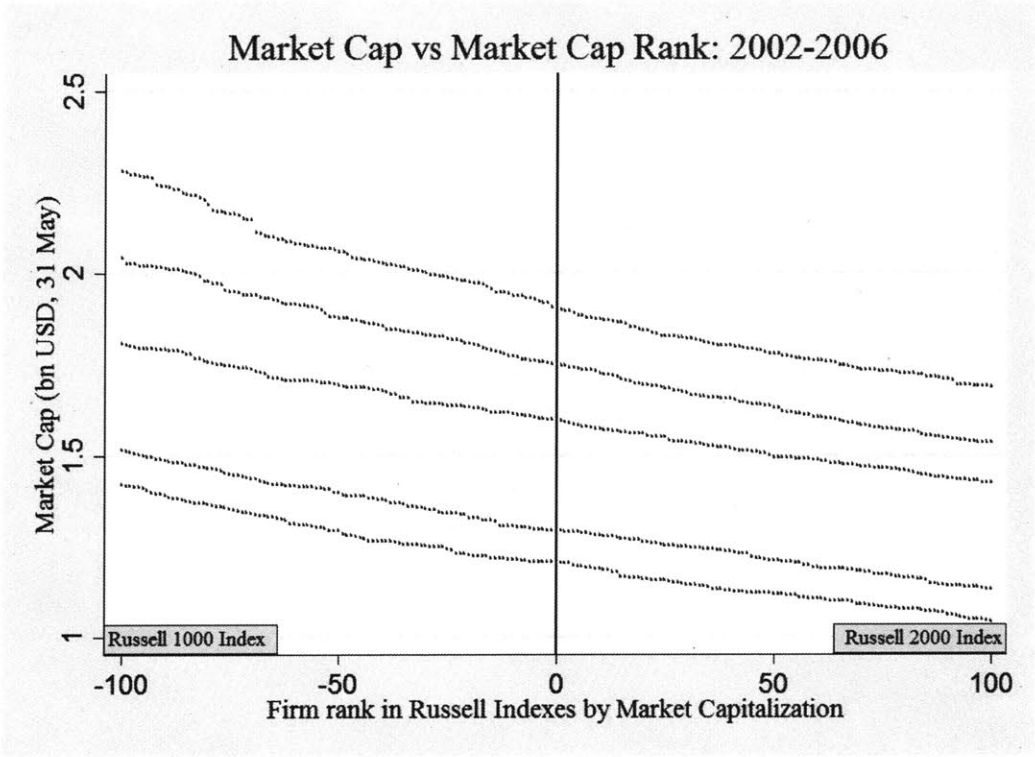
9 Figures and Tables

Figure 1



This Figure presents the assignment variable for the RDD - firms' rank according to a version of Russell's proprietary market capitalization measure - on the X axis, and the market capitalization measure itself on the Y axis. Note that firm rank is here centered on the cutoff between the Russell 1000 and Russell 2000 indices: firms to the left of the line are in the R1000, and firms to the right are in the R2000. A rank of 50 in the graph is equivalent to an uncentered ranking of 1000+50=1050, while a rank of -50 is equivalent to an uncentered rank of 1000-50=950 i.e. the 950th largest firm by market capitalization. Each dot represents a firm, and each line is a year in my sample. The lowest line is 2002, followed in order by each year until the top line, which is 2006. Market capitalization is as of 31 May of each year. The crossing points indicate the market capitalization cutoffs between the indices each year, and range from approximately USD $1.2 to 1.9$ billion. The smoothness of each line as it crosses the threshold at zero (equivalent to an uncentered rank of 1000) is crucial to the RD design, thus this graph contributes prima facie evidence in favor of the validity of the RDD considered here.

RD graphs

The graphs in this section present the main results of the RDD analysis graphically. The X axis presents the assignment variable - Russell's market capitalization rank - in a narrow window centered on the index cutoff, which is placed at rank 1000. To the left of the cutoff firms are in the larger Russell 1000 index of the 1000 largest firms by market capitalization; firms to the right of the cutoff are in the Russell 2000. The specification is locally linear regression in a 100 firm bandwidth on either side of the cutoff, estimated with a triangle kernel. For clarity regarding the underlying data no year fixed effects are included. The lines are estimated using the full un-binned data, and there is also a superimposed scatterplot of the data's average value in bins of the assignment variable (bins are either 10 or 15 rank positions in size). The dashed lines represent one standard error bounds. It is also worth highlighting that for graphs using the up sample (CEO compensation) there are relatively few firms far to the left of the graph (because few firms move 200 positions beyond the cutoff in a single year), making the function much less informative about the average position as one moves further left.

Figure 2: RD graphs for Institutional ownership

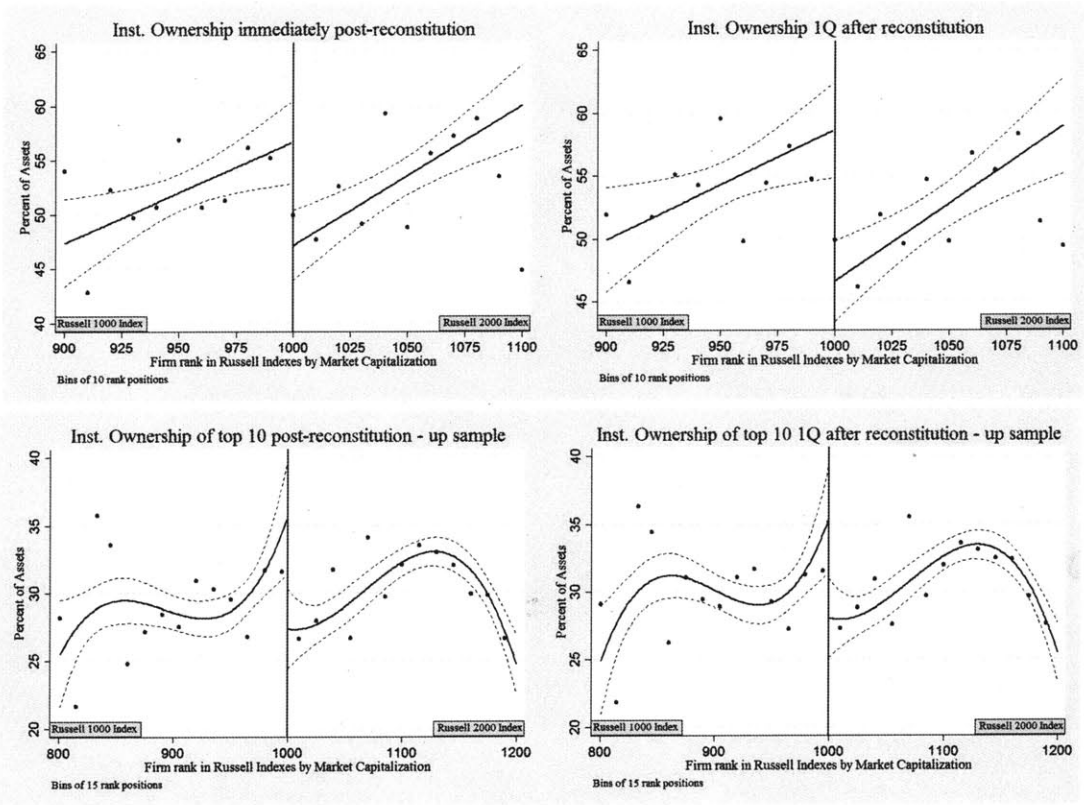


Figure 3: RD graphs for CEO Turnover and Compensation (I)

All CEO compensation graphs shown here are for the up sample: i.e. they compare firms moving up into the Russell 1000 index to firms staying in the Russell 2000.

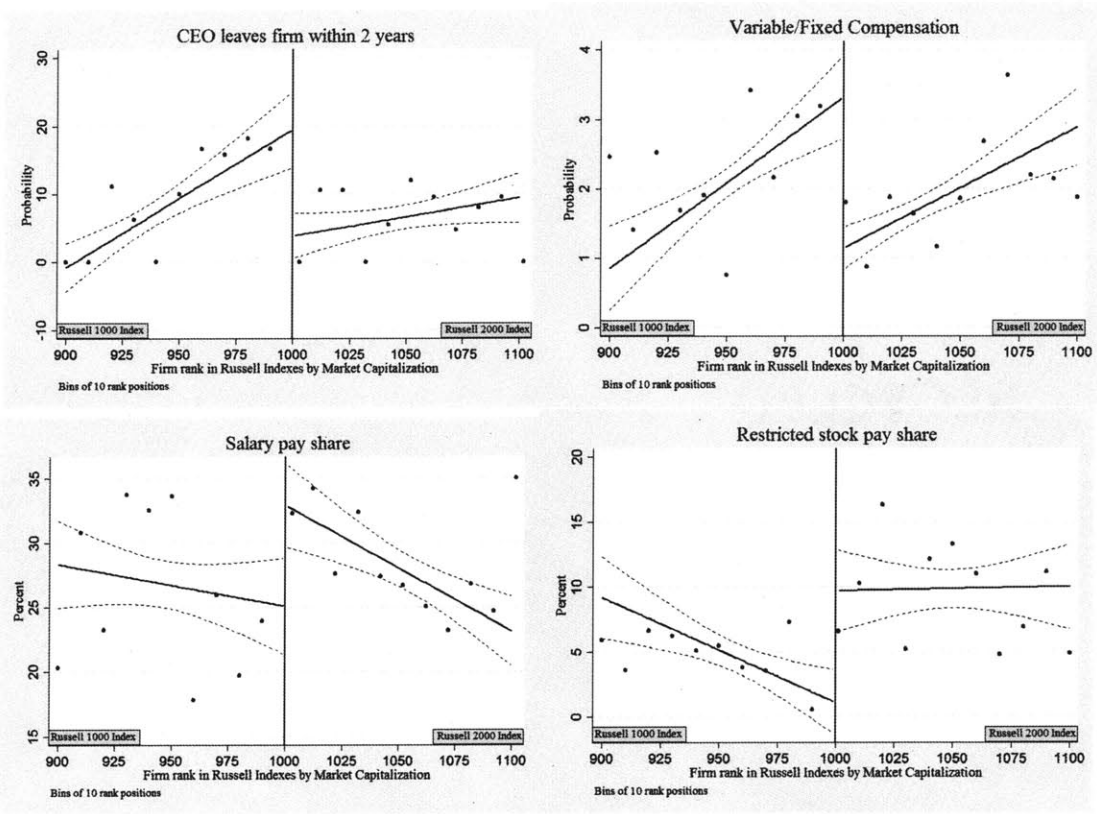


Figure 4: RD graphs for CEO Turnover and Compensation (II)

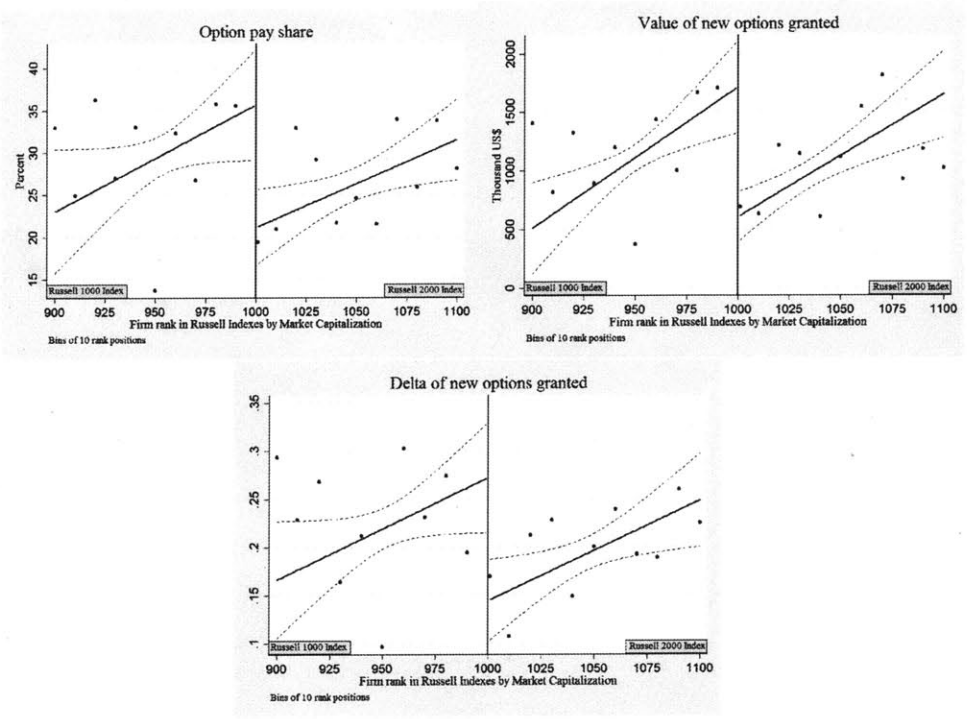


Figure 5: RD graph for Capital Expenditure

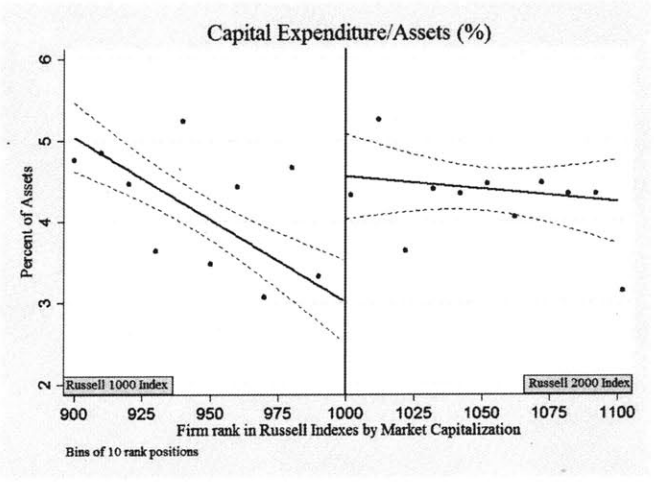


Figure 6: RD graphs for Mergers and Acquisitions

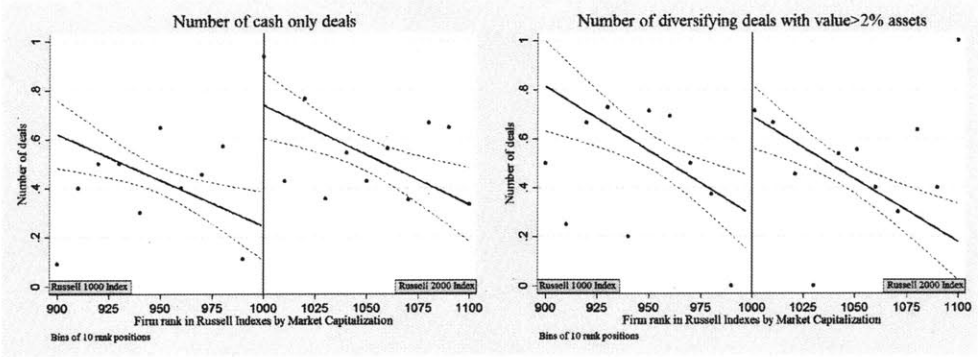
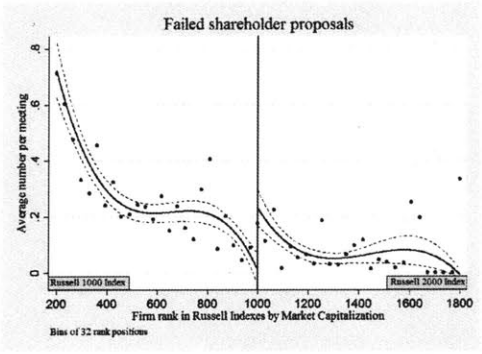


Figure 7: RD graphs for Shareholder Voting



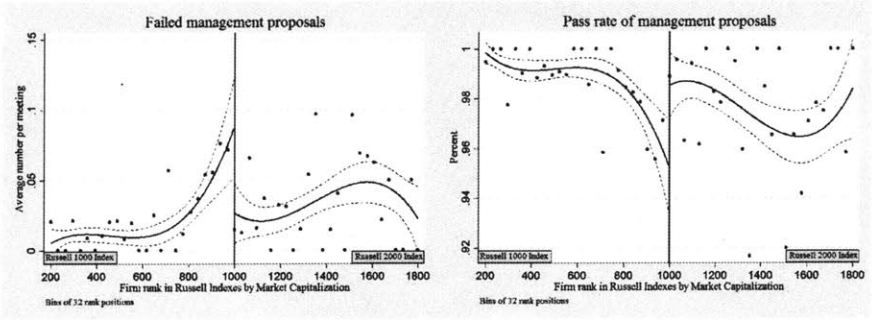


Table 1

Summary Statistics for Institutional Ownership

	Firms in the Russell 1000 index						Firms in the Russell 2000 index						p-value (difference in means)
	Mean	Std. Dev.	10th	Median	90th	Number	Mean	Std. Dev.	10th	Median	90th	Number	
Market capitalization (billion $)	1.67	0.3	1.3	1.7	2.1	500	1.45	0.3	1.1	1.5	1.8	505	**0.00*** **
Total Institutional Ownership (IO) lagged 4 qtrs	48.6%	26.9%	12.9%	47.0%	86.1%	435	49.9%	28.5%	10.3%	49.8%	87.1%	441	0.50
Total IO lagged 3 qtrs	49.3%	27.5%	10.8%	48.1%	86.6%	441	50.4%	29.1%	2.0%	50.3%	88.1%	449	0.55
Total IO lagged 2 qtrs	50.0%	27.4%	12.4%	48.2%	87.1%	443	49.7%	29.2%	0.5%	49.1%	88.0%	450	0.89
Total IO lagged 1 qtr	51.2%	28.5%	9.1%	50.1%	89.2%	451	51.4%	30.0%	0.0%	52.0%	89.7%	456	0.90
Maximum IO lagged 4 qtrs	7.4%	5.3%	2.1%	6.3%	13.9%	435	7.1%	4.6%	1.7%	6.3%	13.4%	441	0.39
Maximum IO lagged 3 qtrs	7.5%	5.3%	2.0%	6.5%	14.1%	441	7.2%	4.8%	1.5%	6.5%	14.0%	449	0.46
Maximum IO lagged 2 qtrs	7.4%	5.1%	2.1%	6.3%	13.6%	443	7.4%	4.7%	1.7%	6.7%	13.9%	450	0.94
Maximum IO lagged 1 qtr	7.5%	5.1%	2.3%	6.4%	13.6%	451	7.5%	4.8%	1.8%	7.0%	14.1%	456	0.92
Total IO of top 10 institutions lagged 4 qtrs	27.9%	14.6%	9.5%	27.3%	48.3%	435	28.4%	15.1%	8.8%	28.5%	48.5%	441	0.61
Total IO of top 10 instutions lagged 3 qtrs	28.4%	14.8%	9.5%	28.4%	49.3%	441	28.8%	15.5%	8.2%	28.7%	49.3%	449	0.73
Total IO of top 10 instutions lagged 2 qtrs	28.4%	14.8%	9.7%	27.0%	48.0%	443	29.0%	15.5%	8.2%	28.5%	50.8%	450	0.55
Total IO of top 10 instutions lagged 1 qtr	29.4%	15.1%	9.8%	28.4%	49.0%	451	29.9%	15.5%	8.8%	30.1%	51.4%	456	0.61
# of IO over 5% lagged 4 qtrs	1.3	1.4	0.0	1.0	3.0	435	1.3	1.4	0.0	1.0	3.0	441	0.82
# of IO over 5% lagged 3 qtrs	1.3	1.4	0.0	1.0	3.0	441	1.4	1.4	0.0	1.0	3.0	449	0.54
# of IO over 5% lagged 2 qtrs	1.4	1.4	0.0	1.0	3.0	443	1.5	1.5	0.0	1.0	3.0	450	0.38
# of IO over 5% lagged 1 qtr	1.4	1.4	0.0	1.0	3.0	451	1.5	1.5	0.0	1.0	3.0	456	0.21
# of IO over 10% lagged 4 qtrs	0.3	0.5	0.0	0.0	1.0	435	0.3	0.6	0.0	0.0	1.0	441	0.77
# of IO over 10% lagged 3 qtrs	0.3	0.6	0.0	0.0	1.0	441	0.3	0.6	0.0	0.0	1.0	449	0.42
# of IO over 10% lagged 2 qtrs	0.3	0.6	0.0	0.0	1.0	443	0.3	0.6	0.0	0.0	1.0	450	0.51
# of IO over 10% lagged 1 qtr	0.3	0.6	0.0	0.0	1.0	451	0.3	0.6	0.0	0.0	1.0	456	0.90
# of "Quasi Indexer" lagged 2 qtrs	88.5	22.5	58.0	89.0	116.0	220	86.9	25.5	55.0	88.0	117.0	221	0.50
# of "Focused Investors" lagged 2 qtrs	4.4	1.6	2.0	4.0	6.0	218	4.5	1.8	2.0	4.0	7.0	219	0.52
# of "Transient Investors" lagged 2 qtrs	46.9	17.4	26.0	46.0	70.0	219	45.4	17.2	23.0	47.0	68.0	218	0.36

The table shows baseline summary statistics for a bandwidth of 100 firms on each side of the index cutoff for the quarters before index reconstitution in the sample period, which occurs near the end of June for the years 2002 to 2006. All variables are calculated from Thomson-Reuters Institutional Holdings (13F) data merged to Russell Indexes data. Total Institutional Ownership (IO) is the sum of all reported ownership of a security (identified by cusip) in the 13F database as a percentage of shares outstanding at the quarter end (obtained from the quarterly CRSP-Compustat Merged Database). Maximum IO is the single highest ownership stake reported by a 13F institution. Total IO of the top 10 institutions it the sum of the ownership of the ten 13F institutions with the largest stake in the firm. Number of IO over 5 (10) percent is the number of 13F institutions that report a stake of over 5% (10%) of the security's outstanding shares. The rightmost column reports the p value of a t test for differences in means between the Russell 1000 and Russell 2000 samples. Market Capitalization is as of May 31, the day index reconstitution is determined based on market capitalization ranking, and is Russell Indexes' proprietary measure.

Table 2

Summary statistics for Blockholders

	Firms in the Russell 1000 index						Firms in the Russell 2000 index						*p*-value (difference in means)
	Mean	Std. Dev.	10th	Median	90th	Number	Mean	Std. Dev.	10th	Median	90th	Number	
Total votes controlled by:													
-all blockholders	18.7%	14%	0%	17%	37%	105	21.2%	13%	6%	20%	38%	101	0.19
-outside blockholders	18.4%	11%	6%	16%	34%	89	20.8%	11%	7%	20%	36%	89	0.15
-inside blockholders	14.3%	10%	5%	9%	28%	23	12.6%	7%	6%	10%	23%	23	0.50
-institutional blockholders	18.1%	11%	6%	15%	32%	87	19.7%	10%	7%	19%	34%	87	0.32
-non-institutional blockholders	15.0%	11%	5%	9%	30%	26	15.3%	12%	6%	12%	29%	28	0.93
-All Officers and Directors (total)	4.4%	3%	1%	4%	9%	105	3.8%	3%	1%	3%	7%	101	0.14
Average votes controlled by:													
-all blockholders	8.8%	3%	6%	8%	13%	92	8.5%	3%	6%	8%	11%	93	0.54
-outside blockholders	8.5%	3%	6%	8%	12%	89	8.0%	3%	6%	8%	11%	89	0.30
-inside blockholders	11.6%	8%	5%	8%	27%	23	11.0%	5%	6%	10%	20%	23	0.76
-institutional blockholders	8.6%	3%	6%	8%	12%	87	7.9%	2%	6%	8%	10%	87	**0.06***
-non-institutional blockholders	10.6%	7%	5%	8%	20%	26	12.2%	10%	6%	10%	22%	28	0.48
Firms with no blockholders	12.4%	33%	0.0	0.0	1.0	105	7.9%	27%	0.0	0.0	0.0	101	0.29
Number of													
-blockholders	2.1	1.5	0.0	2.0	4.0	105	2.4	1.4	1.0	2.0	4.0	101	0.10
-outside blockholders	2.2	1.4	1.0	2.0	4.0	89	2.5	1.1	1.0	2.0	4.0	89	0.11
-inside blockholders	0.3	0.5	0.0	0.0	1.0	103	0.3	0.5	0.0	0.0	1.0	101	0.84
-institutional blockholders	2.1	1.3	1.0	2.0	4.0	87	2.4	1.1	1.0	2.0	4.0	87	0.12
-non-institutional blockholders	1.3	0.5	1.0	1.0	2.0	26	1.2	0.4	1.0	1.0	2.0	28	0.44
Number of 13d blockholders:													
-in total	1.2	1.8	0.0	0.0	4.0	105	1.1	1.6	0.0	0.0	4.0	101	0.65
-outsiders	0.9	1.4	0.0	0.0	3.0	89	0.8	1.3	0.0	0.0	3.0	89	0.74
-insiders	0.4	0.7	0.0	0.0	1.0	103	0.3	0.6	0.0	0.0	1.0	101	0.33
-institutionals	0.9	1.4	0.0	0.0	3.0	87	0.8	1.3	0.0	0.0	3.0	87	0.70
-non-institutionals	0.4	0.7	0.0	0.0	1.0	105	0.4	0.6	0.0	0.0	1.0	101	0.44

The table shows baseline summary statistics for a bandwidth of 100 firms on each side of the index cutoff for the period before index reconstitution. The sample period is index reconstitutions from 2002-2006. All variables are calculated from hand collected SEC Schedule 14A or 14C Proxy filings from the EDGAR database. Outsiders are blockholders that are not also officers or directors; insiders are blockholders with an officer or a director representative. Institutionals are hand coded from the shareholder name and are all financial institutions; Non-Institutionals are blockholders not categorized as Institutionals. All Officers and Directors as a group is the total given in each Proxy filing, and must always be reported, even if the total is below 5%. Number of institutional blockholders is not significantly different between the two groups when a RD is run using the baseline values as the dependent variable (i.e. controlling for market capitalization). 13d Blockholders are blockholders that have filed a Schedule 13d, which is for shareolders with less than 20% of the equity but more than 5%, and when the intent is to "changing or influencing the control of the issuer" of the security. This is understood to mean an intent to exert control of the firm.

Table 3

Summary Statistics for CEO compensation

	Firms in the Russell 1000 index						Firms in the Russell 2000 index						*p*-value (difference in means)
	Mean	Std. Dev.	10th	Median	90th	Number	Mean	Std. Dev.	10th	Median	90th	Number	
	sumstats1	sumstats2	sumstats3	sumstats4	sumstats5	sumstats6	sum sumstats8	sumstats9	sumstats1(	sumstats1	sumstats1:	sumstats1:	sum sumstats15
Variable compensation pay share	57%	27%	7%	63%	85%	336	58%	27%	9%	65%	86%	313	0.69
Variable / Fixed Compensation	2.86	4.95	0.07	1.75	5.89	336	3.18	5.83	0.10	1.84	6.37	313	0.46
Options are part of CEO's pay	66%	48%	0%	100%	100%	341	65%	48%	0%	100%	100%	315	0.94
Option pay share	28%	28%	0%	21%	72%	341	27%	27%	0%	22%	69%	315	0.61
Salary pay share	29%	23%	10%	22%	59%	341	30%	22%	10%	24%	62%	315	0.42
Bonus pay share	16%	20%	0%	10%	42%	341	16%	19%	0%	11%	45%	315	0.77
Stock pay share	10%	22%	0%	0%	47%	341	8%	18%	0%	0%	29%	315	0.15
Restricted stock pay share	8%	19%	0%	0%	33%	341	9%	19%	0%	0%	34%	315	0.52
Long Term Incentive pay share	4%	15%	0%	0%	0%	341	2%	11%	0%	0%	0%	315	0.23
Perks pay share	7%	17%	0%	2%	16%	341	5%	11%	0%	2%	13%	315	**0.03****
Nonequity compensation pay share	8%	19%	0%	0%	28%	341	7%	15%	0%	0%	28%	315	0.30
Total pay ($000)	3,830	4,057	895	2,886	7,106	347	3,336	3,560	941	2,659	5,647	322	**0.10***
New option grants ($000)	1,077	1,475	0	632	2,867	341	1,232	2,800	0	550	2,816	315	0.37
New option Delta	0.21	0.25	0.00	0.14	0.55	338	0.19	0.24	0.00	0.11	0.50	313	0.46

The table shows baseline summary statistics for a bandwidth of 100 firms on each side of the index cutoff for the period before index reconstitution. The sample period is centered on index reconstitutions ocurring in June for yeas 2002-2006 inclusive. All variables are calculated from Execucomp data merged to Russell Indexes data. Options equals option_awards_fv if present, and option_awards_blk_value if not. Variable compensation payshare equals (bonus + options + stock_awards_fv+rstkgrnt)/tdc1. Variable over fixed compensation is (bonus + options + stock_awards_fv+rstkgrnt) / (tdc1-numerator). Any options in pay package is an indicator equal to 1 if the option variable is not missing or zero. Pay share variables are the respective variables (options, salary, bonus, stock_awards_fv, rstkgrnt, ltip, othcomp, noneq_incent) divided by tdc1. Total pay is tdc1; new option grants is options; new option delta is calculated following Core and Guay (2002). Pay shares sum exactly to one for all observations in the sample, so missing components

Table 4

Summary Statistics for Accounting variables

	Firms in the Russell 1000 index						Firms in the Russell 2000 index						p-value (difference in means)
	Mean	Std. Dev.	10th	Median	90th	# obs	Mean	Std. Dev.	10th	Median	90th	# obs	
Operating Income/ Assets	12.6%	9.9%	2.1%	11.6%	25.4%	438	12.8%	10.4%	2.1%	12.2%	26.2%	447	0.76
Profits/ Enterprise Value	1.8%	4.9%	-1.2%	2.5%	5.1%	476	1.7%	4.9%	-2.6%	2.6%	5.2%	480	0.72
Sales Growth	11.9%	22.6%	-10.1%	7.7%	39.8%	465	10.0%	22.8%	-9.3%	6.2%	37.5%	465	0.22
Capex/ Assets	4.4%	4.8%	0.0%	3.1%	10.1%	446	4.5%	4.9%	0.1%	3.1%	10.9%	450	0.70
Capx/ Ent. Value	2.7%	3.1%	0.0%	1.7%	6.7%	446	2.7%	3.1%	0.1%	1.7%	6.5%	450	0.93
PPE/ Assets	25.2%	23.5%	1.4%	17.8%	64.5%	439	25.3%	24.2%	1.0%	17.1%	65.4%	448	0.94
R&D/ Sales	16.7%	36.8%	1.0%	5.0%	36.7%	180	20.0%	57.8%	0.7%	5.2%	31.6%	171	0.52
Debt/ (Debt+Equity)	25.6%	22.4%	0.0%	21.5%	59.2%	476	26.1%	23.1%	0.0%	21.3%	60.1%	480	0.70
Financial Debt/ Assets	24.9%	20.7%	0.0%	22.2%	53.1%	476	25.8%	20.8%	0.0%	24.8%	53.9%	480	0.50
Financial Debt/ Ent. Value	18.3%	16.5%	0.0%	14.7%	41.8%	476	19.0%	17.3%	0.0%	15.6%	43.9%	480	0.55
Balance Sheet Leverage	56.8%	23.8%	22.7%	58.0%	90.7%	475	57.1%	24.2%	22.1%	56.9%	91.5%	479	0.85
Debt issuance / Assets	13.8%	20.0%	0.0%	4.3%	45.5%	421	13.9%	19.9%	0.0%	2.9%	45.3%	416	0.95
Debt retirement / Assets	10.7%	14.0%	0.0%	4.1%	32.6%	432	10.2%	14.1%	0.0%	3.3%	32.6%	428	0.65
Dividend Yield	1.6%	2.4%	0.0%	0.4%	4.9%	472	1.6%	2.5%	0.0%	0.5%	4.9%	480	0.93
Repurchases (% of equity)	1.7%	3.1%	0.0%	0.0%	6.1%	414	2.1%	3.8%	0.0%	0.3%	7.8%	408	**0.05****
Market-to-Book	1.9	1.3	1.0	1.4	3.7	476	1.9	1.3	1.0	1.4	3.5	480	0.83
Q	2.0	1.3	1.0	1.5	3.9	419	2.1	1.4	1.1	1.5	4.0	404	0.33
Enterprise Value ($m)	4,214	3,683	1,608	2,906	9,561	476	3,752	3,414	1,436	2,513	8,010	480	**0.04****
Enterprise Value (%Δ)	17.3%	38.4%	-14.9%	9.9%	61.5%	465	16.3%	35.7%	-21.1%	9.3%	60.3%	465	0.70

Summary statistics for a 100 firm bandwidth on either side of the index cutoff. The sample period is centered on Index reconstitutions ocurring in June for years 2002-2006 inclusive. All variables are calculated from the CRSP-Compustat Merged Database merged to Russell Indexes data. Operating income/Assets is oibdp/at; Profits over enterprise value is ib / enterprise value, where the latter is at - seq -pstock + txditc + csho*prcc_f. Sales growth is the 1 year change in sale. Capex is capx, PPE is ppent, R&D/Sales is xrd/sale. Debt/(Debt+Equity) is (dltt+dlc)/[dltt+dlc +csho*prcc_f]. Financial Debt/Assets is (dltt+dlc)/at. Balance Sheet leverage is total liabilities/at. Debt issuance is dltis, debt retirement is dltr. Dividend yield is (dvc+dvp)/(csho*prcc_f). Repurchases are prstkc/(shout*prcc_f). Market to book is enterprise value/at. Q is (prcc_f*csho + at - ceq - txdb)/at. Repurchases are not significant in a RDD run with the baseline values as the dependent variable (i.e. controlling for the assignment variable, market capitalization rank).

Table 5

Summary statistics for Mergers and Acquisitions

	Firms in the Russell 1000 index						Firms in the Russell 2000 index						p-value (difference in means)
	Mean	Std. Dev.	10th	Median	90th	Number	Mean	Std. Dev.	10th	Median	90th	Number	
% of firms with no deals	0.63	0.48	0.00	1.00	1.00	479	0.65	0.48	0.00	1.00	1.00	476	0.50
# deals per acquirer	1.57	0.98	1.00	1.00	3.00	178	1.77	1.45	1.00	1.00	3.00	167	0.12
# of public targets	0.09	0.28	0.00	0.00	0.00	279	0.06	0.25	0.00	0.00	0.00	296	0.29
# of private targets	0.57	0.50	0.00	1.00	1.00	279	0.58	0.49	0.00	1.00	1.00	296	0.75
# of subsidiary targets	0.34	0.48	0.00	0.00	1.00	279	0.34	0.47	0.00	0.00	1.00	296	0.89
# of deals with all cash payment	0.26	0.44	0.00	0.00	1.00	279	0.33	0.47	0.00	0.00	1.00	296	0.13
# of deals involving stock payment	0.54	0.50	0.00	1.00	1.00	279	0.48	0.50	0.00	0.00	1.00	296	0.31
# of deals with all stock payment	0.02	0.15	0.00	0.00	0.00	279	0.02	0.14	0.00	0.00	0.00	296	0.93
# of diversifying acquisitions	0.56	0.50	0.00	1.00	1.00	279	0.55	0.50	0.00	1.00	1.00	296	0.81
# of domestic targets	0.82	0.39	0.00	1.00	1.00	279	0.86	0.35	0.00	1.00	1.00	296	0.25
Total value of all acquisitions in year (% of assets)	12.5	16.6	1.3	7.2	28.5	103	20.7	55.3	2.1	6.1	51.9	105	0.15
Average value of all acquisitions in year (% of assets)	11.5	16.6	1.3	6.0	25.6	103	15.8	33.0	1.9	5.1	41.6	105	0.23
Deal value ($ million)	216	476	6	67	481	153	203	536	8	61	425	170	0.82
# deals >2% of assets	0.62	0.49	0.00	1.00	1.00	153	0.65	0.48	0.00	1.00	1.00	170	0.59
# deals >3% of assets	0.51	0.50	0.00	1.00	1.00	153	0.52	0.50	0.00	1.00	1.00	170	0.90
# deals >10% of assets	0.22	0.42	0.00	0.00	1.00	153	0.22	0.41	0.00	0.00	1.00	170	0.92
# diversifying deals >2% of assets	0.35	0.48	0.00	0.00	1.00	153	0.31	0.46	0.00	0.00	1.00	170	0.56
# diversifying deals >3% of assets	0.29	0.45	0.00	0.00	1.00	153	0.25	0.43	0.00	0.00	1.00	170	0.47
# diversifying deals >10% of assets	0.12	0.32	0.00	0.00	1.00	153	0.09	0.28	0.00	0.00	0.00	170	0.37
# deals >$50million	0.57	0.50	0.00	1.00	1.00	153	0.55	0.50	0.00	1.00	1.00	170	0.81
# deals >$100million	0.42	0.50	0.00	0.00	1.00	153	0.38	0.49	0.00	0.00	1.00	170	0.48
# deals >$200million	0.22	0.41	0.00	0.00	1.00	153	0.22	0.42	0.00	0.00	1.00	170	0.87
# deals >$400million	0.14	0.35	0.00	0.00	1.00	153	0.11	0.32	0.00	0.00	1.00	170	0.40

The table shows baseline summary statistics for a bandwidth of 100 firms on each side of the index cutoff for the period before index reconstitution. The sample period is centered on index reconstitutions ocurring in June for years 2002-2006 inclusive. All variables are calculated from SDC platinum data merged to Russell Indexes data, except for firm assets, which is obtained from the CRSP-Compustat Merged Database. SDC classifies targets as public (publicly listed firms), private (unlisted firms), and subsidiaries (over 50% is owned by a parent entity, which may itself be publicly listed). Diversifying deals are deals in which the primary 3 digit SIC code of the acquirer does not match that of the target. All deals in the sample have either a US based acquirer or US based ultimate parent of the acquirer. The rightmost column reports the p value of a t test for differences in means between the Russell 1000 and Russell 2000 samples.

Table 6

Summary Statistics for Voting at Shareholder Meetings

	Firms in the Russell 1000 index						Firms in the Russell 2000 index						p-value (difference in means)
	Mean	Std. Dev.	10th	Median	90th	# obs	Mean	Std. Dev.	10th	Median	90th	# obs	
Avg. votes for Management Proposals	82%	15%	58%	86%	98%	85	81%	15%	59%	85%	97%	82	0.79
Avg. votes for Shareholder Proposals	41%	22%	9%	32%	72%	19	46%	25%	8%	51%	73%	22	0.50
# of proposals per meeting	1.6	1.0	1.0	1.0	3.0	97	1.7	0.9	1.0	1.0	3.0	93	0.30
# of Management Proposals per meeting	1.3	0.8	0.0	1.0	2.0	97	1.3	0.8	0.0	1.0	2.0	93	0.64
Pass rate of Management Proposals	95%	19%	100%	100%	100%	85	96%	19%	100%	100%	100%	82	0.99
# of Shareholder Proposals per meeting	0.27	0.7	0.0	0.0	1.0	97	0.35	0.8	0.0	0.0	1.0	93	0.42
Pass rate of Shareholder Proposals	32%	45%	0%	0%	100%	19	44%	50%	0%	0%	100%	22	0.42
Meetings with Shareholder Proposals	0.20	0.40	0.00	0.00	1.00	97	0.24	0.43	0.00	0.00	1.00	93	0.50
# of failed Management Proposals per meeting	0.05	0.22	0.00	0.00	0.00	97	0.05	0.23	0.00	0.00	0.00	93	0.95
# of failed Shareholder Proposals per meeting	0.18	0.46	0.00	0.00	1.00	97	0.22	0.64	0.00	0.00	1.00	93	0.62

The table shows baseline summary statistics for a bandwidth of 150 firms on each side of the index cutoff for the period before index reconstitution. The sample period is centered around reconstitutions ocurring in June for years 2002-2006 inclusive. Almost identical results obtain with a bandwidth of 100 firms, but the 150 firm bandwidth is chosen to fit the robustness sample bandwidth. The p-values are from t-tests for difference in means. All variables are calculated from RiskMetrics data merged to Russell Indexes data. Average votes for management (shareholder) proposals is the votes_for variable in the RiskMetrics data taken only for proposals submitted by management (shareholders: Irrc issue codes 2000 or over) and averaged across all shareholders proposals at each meeting. Number of proposals per meeting is the number of proposals recorded in the RiskMetrics data for each meeting-sponsor combination. Pass rate of management proposals is the number of management proposals that are recorded as passed divided by the total number of management proposals. Meetings with shareholder proposals is an indicator that takes a value of one if at least one shareholder proposal is recorded by RiskMetrics. Number of failed shareholder (management) proposals is the number of proposals per Annual Shareholder Meeting that fail to pass and are sponsored by shareholders (management).

Table 7.1 - Institutional Ownership (Total)

	Ownership by all institutions Total Institutional Ownership						Ownership by all institutions Total equity ownership of Top 10 institutions					
	June (Reconstit.)	Sept.	Dec.	Mar. (t+1)	June (t+1)	Sept. (t+1)	June (Reconstit.)	Sept.	Dec.	Mar. (t+1)	June (t+1)	Sept. (t+1)
R2000 vs R1000												
Coefficient	**10.2%***	**12%****	7.1%	**9.6%***	7.9%	7.8%	3.5%	2.1%	2.6%	**4.7%***	**6.6%****	**8.2%*****
s.e.	5.4%	5.5%	5.7%	5.9%	6.1%	6.2%	2.8%	2.8%	3.0%	2.8%	2.9%	3.0%
p-value	0.06	0.03	0.21	0.10	0.20	0.21	0.21	0.45	0.39	0.10	0.03	0.01
# obs.	905	900	886	872	860	846	905	900	886	872	860	846
# clusters	521	519	513	505	498	491	521	519	513	505	498	491
STAY R2000 vs UP												
Coefficient	**15.3%****	**18%****	**13.2%***	**16.8%****	**13.6%***	9.7%	**7.9%****	**6.4%***	**7.0%***	**9.1%*****	**9.4%****	**11.2%*****
s.e.	7.4%	7.5%	7.8%	8.2%	8.3%	8.3%	3.7%	3.6%	3.8%	3.7%	3.9%	3.9%
p-value	0.04	0.02	0.09	0.04	0.10	0.24	0.03	0.08	0.06	0.01	0.02	0.00
# obs.	534	531	523	513	507	498	534	531	523	513	507	498
# clusters	387	385	381	374	369	362	387	385	381	374	369	362
DOWN vs STAY R1000												
Coefficient	4.3%	4.0%	1.0%	0.9%	0.5%	7.2%	-0.6%	-2.5%	-1.5%	0.4%	4.1%	5.6%
s.e.	8.6%	8.7%	9.0%	9.4%	10.5%	10.8%	4.6%	4.6%	4.9%	4.7%	4.9%	5.1%
p-value	0.62	0.64	0.91	0.93	0.96	0.51	0.90	0.59	0.76	0.93	0.41	0.27
# obs.	355	353	347	343	337	332	355	353	347	343	337	332
# clusters	264	263	259	255	251	250	264	263	259	255	251	250

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff, in the period following Index reconstitution. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000.All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on reconstitutions from June 2002 to June 2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and Thomson Reuters 13F. Total IO is total Institutional ownership in the firm as a percent of outstanding equity by institutions in the 13F database. Ownership by top 10 is ownership by the ten institutions with the largest holdings in that security. Coefficients significant at ten percent or below are in bold; ***, **, * denote statistical significance at the 1%, 5% and 10% respectively.

Table 7.2 - Institutional Ownership (Top 10 institutions - "Indexers" and "Quasi Indexers")

	Ownership by "Indexers" Total equity ownership of Top 10 institutions						Ownership by "Quasi Indexers" Total equity ownership of Top 10 institutions					
	June (Reconstit.)	Sept.	Dec.	Mar. (t+1)	June (t+1)	Sept. (t+1)	June (Reconstit.)	Sept.	Dec.	Mar. (t+1)	June (t+1)	Sept. (t+1)
R2000 vs R1000												
Coefficient	3.4%	2.2%	2.1%	**4.4%***	**5.3%****	**6.4%*****	2.5%	1.6%	1.4%	3.8%	**5.6%****	**7.3%*****
s.e.	2.3%	2.3%	2.4%	2.4%	2.4%	2.5%	2.4%	2.3%	2.4%	2.4%	2.5%	2.5%
p-value	0.14	0.35	0.38	0.06	0.03	0.01	0.29	0.51	0.56	0.12	0.02	0.00
# obs.	905	900	886	872	860	846	900	895	881	867	855	841
# clusters	521	519	513	505	498	491	517	515	509	501	494	487
STAY R2000 vs UP												
Coefficient	**5.8%****	**4.6%***	**5.1%***	**7.5%*****	**7.0%****	**8.4%*****	**6.7%****	**5.6%***	**5.1%***	**8.2%*****	**8.5%*****	**10.1%*****
s.e.	3.0%	3.0%	3.1%	3.0%	3.1%	3.1%	3.0%	2.9%	3.0%	3.1%	3.3%	3.3%
p-value	0.05	0.10	0.10	0.01	0.03	0.01	0.03	0.06	0.09	0.01	0.01	0.00
# obs.	534	531	523	513	507	498	531	528	520	510	504	495
# clusters	387	385	381	374	369	362	384	382	378	371	366	359
DOWN vs STAY R1000												
Coefficient	1.1%	-0.7%	-0.6%	0.7%	3.0%	3.8%	1.5%	-2.5%	-2.3%	-1.0%	3.3%	5.4%
s.e.	3.8%	3.8%	4.0%	3.9%	4.0%	4.2%	4.1%	4.1%	4.2%	4.0%	4.2%	4.3%
p-value	0.77	0.86	0.87	0.85	0.45	0.37	0.72	0.54	0.58	0.80	0.44	0.21
# obs.	355	353	347	343	337	332	353	351	345	341	335	330
# clusters	264	263	259	255	251	250	262	261	257	253	249	248

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000.All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on reconstitutions from June 2002 to June 2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and Thomson Reuters 13F. Total ownership of top 10 institutions is ownership by the ten institutions with the largest holdings in that security by category (i.e. Indexers, Quasi Indexers). Ownership by "Quasi-Indexers" is ownership by institutions classified as such by Bushee (2001), i.e. they have high diversification and low turnover. Ownership by "Indexers" is ownership by investment companies and independent investment advisors (determined from Thomson Reuters typecodes as extended by Brian Bushee) that are also classified as Quasi-Indexers. Coefficients significant at ten percent or below are in bold; ***, **, * denote statistical significance at the 1%, 5% and 10% respectively.

Table 7.3 - Institutional Ownership (Top 10 institutions - "Focused investors" and "Transient investors")

	Ownership by "Focused Investors" Total equity ownership of Top 10 institutions						Ownership by "Transient Investors" Total equity ownership of Top 10 institutions					
	June (Reconstit.)	Sept.	Dec.	Mar. (t+1)	June (t+1)	Sept. (t+1)	June (Reconstit.)	Sept.	Dec.	Mar. (t+1)	June (t+1)	Sept. (t+1)
R2000 vs R1000												
Coefficient	**5.6%***	**4.7%***	4.4%	**7.5%****	**9.4%*****	**8.4%*****	**6.4%****	**6.8%*****	**4.5%***	4.6%	**5.3%***	**8.5%*****
s.e.	2.8%	2.8%	2.8%	3.2%	3.3%	3.3%	2.7%	2.6%	2.8%	2.9%	3.0%	2.9%
p-value	0.06	0.10	0.12	0.02	0.00	0.01	0.02	0.01	0.10	0.12	0.08	0.00
# obs.	898	887	874	864	852	832	901	891	879	867	855	839
# clusters	515	511	504	498	492	483	517	513	507	500	494	486
STAY R2000 vs UP												
Coefficient	**8.5%*****	**7.4%***	5.4%	**10.6%*****	**13.3%*****	**10.5%*****	**8.5%****	**9.2%*****	**7.8%****	**9.9%****	**9.1%****	**11.0%*****
s.e.	3.5%	3.8%	3.6%	4.0%	4.1%	4.2%	3.5%	3.5%	3.6%	4.1%	4.1%	4.0%
p-value	0.01	0.05	0.14	0.01	0.00	0.01	0.02	0.01	0.03	0.02	0.03	0.01
# obs.	530	523	516	509	503	489	532	526	519	511	505	494
# clusters	383	380	375	370	365	355	385	382	378	372	367	359
DOWN vs STAY R1000												
Coefficient	0.5%	0.9%	3.1%	4.3%	4.1%	5.7%	5.1%	5.4%	1.0%	-1.8%	1.5%	6.1%
s.e.	4.9%	4.7%	5.0%	5.4%	5.6%	5.8%	4.4%	4.4%	4.7%	4.7%	4.9%	4.7%
p-value	0.91	0.85	0.53	0.43	0.46	0.32	0.25	0.22	0.83	0.69	0.76	0.20
# obs.	352	348	342	339	333	327	353	349	344	340	334	329
# clusters	261	258	254	251	247	246	262	259	256	252	248	247

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000.All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on reconstitutions from June 2002 to June 2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and Thomson Reuters 13F. Total ownership of top 10 institutions is ownership by the ten institutions with the largest holdings in that security by category (i.e. Focused and Transient Investors). Ownership by "Focused Investors" and "Transient Investors" is ownership by institutions classified as "Dedicated" and "Transient" respectively by Bushee (2001), i.e. they have low diversification with low turnover, and high diversification with high turnover respectively. Coefficients significant at ten percent or below are in bold; ***, **, * denote statistical significance at the 1%, 5% and 10% respectively.

Table 8 - Blockholder ownership

R2000 vs R1000	Total BH votes	Outsider BH votes	Insider BH votes	Institutional BH votes	Non-institutional BH votes	Total votes of Officers and Directors	Firms without BH	# BH	# Outside BH	# Inside BH	# of Institutional BH	# of non-Institutional BH	# of 13d BH
Coefficient	7.0%	1.4%	9.4%	0.8%	8.7%	-1.0%	**-0.15***	**0.77***	0.39	0.13	0.25	0.39	0.41
s.e.	4.6%	4.4%	8.0%	4.6%	8.4%	1.0%	0.08	0.45	0.46	0.14	0.46	0.24	0.65
p-value	0.13	0.75	0.24	0.86	0.31	0.35	0.06	0.09	0.39	0.38	0.58	0.11	0.53
# obs.	252	222	59	219	66	252	252	252	222	250	219	66	252
# clusters	150	139	39	139	44	150	150	150	139	150	139	44	150

STAY R2000 vs UP	Total BH votes	Outsider BH votes	Insider BH votes	Institutional BH votes	Non-institutional BH votes	Total votes of Officers and Directors	Firms without BH	# BH	# Outside BH	# Inside BH	# of Institutional BH	# of non-Institutional BH	# of 13d BH
Coefficient	8.8%	1.7%	-3.4%	1.0%	-2.2%	-1.2%	**-0.26***	**1.32***	0.76	0.18	0.57	0.33	0.21
s.e.	6.8%	6.0%	13.0%	6.1%	11.6%	1.7%	0.13	0.70	0.69	0.23	0.69	0.35	1.05
p-value	0.20	0.77	0.80	0.87	0.85	0.49	0.05	0.06	0.28	0.42	0.41	0.35	0.84
# obs.	143	124	34	122	38	143	143	143	124	141	122	38	143
# clusters	108	97	28	96	32	108	108	108	97	108	96	32	108

DOWN vs STAY R1000	Total BH votes	Outsider BH votes	Insider BH votes	Institutional BH votes	Non-institutional BH votes	Total votes of Officers and Directors	Firms without BH	# BH	# Outside BH	# Inside BH	# of Institutional BH	# of non-Institutional BH	# of 13d BH
Coefficient	6.5%	2.0%	**0.5%***	0.4%	**0.5%****	-0.4%	-0.09	0.35	0.14	-0.01	-0.06	**0.01****	0.72
s.e.	7.0%	6.9%	0.2%	6.9%	0.2%	1.1%	0.13	0.67	0.64	0.20	0.64	0.00	0.88
p-value	0.35	0.77	0.05	0.95	0.02	0.73	0.47	0.60	0.83	0.96	0.93	0.01	0.42
# obs.	109	98	25	97	28	109	109	109	98	109	97	28	109
# clusters	86	79	21	78	23	86	86	86	79	86	78	23	86

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1K) and Russell 2000 (R2K) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1K (of larger firms) instead of the R2K among firms close to the cutoff in the period following reconstitution. The first panel, labelled R1000 vs R2000, compares all firms in the R1K and within the bandwidth against all firms in the R2K and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1K to those that remain in the R2K. The third panel compares firms in the bandwidth that move down into the R2K with those that remain in the R1K.All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is index reconstitutions from 2002-2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and SEC's EDGAR database. Outsiders are blockholders that are not also officers or directors; insiders are blockholders with an officer or a director representative. Institutionals are hand coded from the shareholder name and are all financial institutions; Non-Institutionals are blockholders not categorized as Institutionals. All Officers and Directors as a group is the total given in each Proxy filing, and must always be reported, even if the total is below 5%. Coefficients significant at ten percent or below are in bold; p values are displayed in the table.

Table 9 - CEO Compensation

R2000 vs R1000	CEO leaves firm within year	CEO leaves firm within 2 years	Variable/ Fixed Compensation	Option pay share	Salary pay share	Restricted Stock pay share	LongTerm Incentive pay share	Total pay ($000)	New option grants ($000)	New option Delta
Coefficient	-3.9%	6.1%	**1.29***	7.3%	**-9.5%****	-1.8%	-2.2%	239	513	0.07
s.e.	6.0%	6.3%	0.7	6.2%	4.9%	3.5%	2.8%	675	386	0.06
p-value	0.51	0.33	0.07	0.24	0.05	0.60	0.42	0.72	0.18	0.27
# obs.	679	679	643	650	650	650	650	662	650	645
# clusters	386	386	369	372	372	372	372	377	372	369

STAY R2000 vs UP	CEO leaves firm within year	CEO leaves firm within 2 years	Variable/ Fixed Compensation	Option pay share	Salary pay share	Restricted Stock pay share	LongTerm Incentive pay share	Total pay ($000)	New option grants ($000)	New option Delta
Coefficient	-5.4%	**17.7%****	**1.74****	**16.7%***	**-10.9%***	**-9.6%****	**-9.1%****	887	**1156****	**0.14***
s.e.	6.6%	8.7%	0.9	8.9%	6.0%	4.5%	3.9%	941	489	0.08
p-value	0.41	0.04	0.05	0.06	0.07	0.03	0.02	0.35	0.02	0.07
# obs.	397	397	376	378	378	378	378	386	378	378
# clusters	290	290	275	276	276	276	276	282	276	276

DOWN vs STAY R1000	CEO leaves firm within year	CEO leaves firm within 2 years	Variable/ Fixed Compensation	Option pay share	Salary pay share	Restricted Stock pay share	LongTerm Incentive pay share	Total pay ($000)	New option grants ($000)	New option Delta
Coefficient	-3.5%	-7.5%	0.5	-5.9%	-7.3%	7.8%	6.1%	-890	-321	-0.04
s.e.	11.0%	9.3%	1.3	9.2%	8.7%	5.8%	4.0%	989	688	0.10
p-value	0.75	0.42	0.68	0.52	0.40	0.18	0.13	0.37	0.64	0.66
# obs.	281	281	266	271	271	271	271	275	271	266
# clusters	210	210	199	203	203	203	203	206	203	199

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff in the period after reconstitution. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000.All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on reconstitutions occurring in years 2002 to 2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and Execucomp. CEO leaves firm variables are indicators equal to one if Execucomp records the CEO as having left the firm in the July-June30 period following reconstitution (or two such periods for the 2 year variable). Options equals option_awards_fv if present, and option_awards_blk_value if not. Variable over fixed compensation is (bonus + options + stock_awards_fv+rstkgrnt) / (tdc1-numerator). Options are part of CEO's pay is an indicator equal to 1 if options is not missing or zero. Pay share variables are the respective variables (options, salary, rstkgrnt, ltip) divided by tdc1. Total pay is tdc1; new option grants is options; new option delta is calculated following Core and Guay (2002). Pay shares sum exactly to one for all observations in the sample, so missing components are set to zero. Coefficients significant at ten percent or below are in bold; p values are displayed in the table.

Table 10 - Accounting variables

R2000 vs R1000	Capex/ Assets	R&D/ Sales	Debt/ (Debt + Equity)	Fin. Debt/ Assets	Debt Issuance /Assets	Debt retirement /Assets	Dividend Yield	Repurchases as % of equity	Op. Profits/ Assets	Market-to-book
Coefficient	**-2.04%****	7.9%	-5.5%	-4.3%	-4.3%	-2.2%	0.2%	-0.1%	-2.3%	0.3%
s.e.	1.0%	5.0%	5.0%	3.6%	2.9%	2.3%	0.5%	0.9%	2.2%	0.2%
p -value	0.04	0.11	0.27	0.23	0.13	0.32	0.67	0.87	0.29	0.22
# obs.	877	473	880	880	869	864	875	837	825	880
# clusters	521	283	521	521	516	516	521	500	491	521

STAY R2000 vs UP	Capex/ Assets	R&D/ Sales	Debt/ (Debt + Equity)	Fin. Debt/ Assets	Debt Issuance /Assets	Debt retirement /Assets	Dividend Yield	Repurchases as % of equity	Op. Profits/ Assets	Market-to-book
Coefficient	**-2.51%***	10.7%	-4.2%	-3.9%	-5.4%	-2.6%	0.8%	0.0%	-1.8%	0.5%
s.e.	1.4%	8.2%	6.4%	4.3%	3.9%	3.0%	0.7%	1.2%	3.2%	0.3%
p -value	0.07	0.20	0.51	0.37	0.17	0.39	0.24	0.98	0.58	0.14
# obs.	516	268	519	519	512	508	515	493	481	519
# clusters	381	204	381	381	377	375	381	367	354	381

DOWN vs STAY R1000	Capex/ Assets	R&D/ Sales	Debt/ (Debt + Equity)	Fin. Debt/ Assets	Debt Issuance /Assets	Debt retirement /Assets	Dividend Yield	Repurchases as % of equity	Op. Profits/ Assets	Market-to-book
Coefficient	-1.7%	7.7%	-5.5%	-3.8%	-2.0%	-0.8%	-0.7%	-0.4%	-3.6%	0.0%
s.e.	1.5%	5.7%	8.3%	6.1%	5.0%	4.2%	0.7%	1.5%	3.2%	0.3%
p -value	0.27	0.18	0.50	0.54	0.70	0.84	0.30	0.77	0.26	0.87
# obs.	343	200	343	343	339	338	342	327	328	343
# clusters	261	150	261	261	259	261	260	252	250	261

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000, among firms close to the cutoff, averaged over the two years following index reconstitution. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000. All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on reconstitutions occurring June 2002 to June 2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and Crsp-Compustat. Capex is capx. R&D/Sales is xrd/sale. Debt/(Debt+Equity) is (dltt+dlc)/[dltt+dlc +csho*prcc_f]. Financial Debt/Ent. Value is (dltt+dlc)/Ent. value; Ent. value is at - seq -pstock + txditc + csho*prcc_f. Debt issuance is dltis, debt retirement is dltr. Dividend yield is (dvc+dvp)/(csho*prcc_f). Repurchases are prstkc/(shout*prcc_f). Op. profits /Assets is oibdp/at; Market to book is enterprise value/at.

Table 11 - Mergers and Acquisitions

R2000 vs R1000	# deals per acquirer	# cash	# diversifying	# over2% +div	# over3% +div	# over10% +div	# >$50m
Coefficient	-0.47	**-0.53****	**-0.49***	**-0.45****	**-0.38***	**-0.26***	-0.24
s.e.	0.30	0.22	0.28	0.22	0.22	0.15	0.22
p value	0.11	0.02	0.09	0.04	0.09	0.08	0.29
# obs.	342	342	342	206	206	206	206
# clusters	259	259	259	174	174	174	174
Only non-missing deal values	N	N	N	Y	Y	Y	Y

STAY R2000 vs UP	# deals per acquirer	# cash	# diversifying	# over2% +div	# over3% +div	# over10% +div	# >$50m
Coefficient	0.01	**-0.52****	-0.14	-0.36	-0.20	-0.16	-0.05
s.e.	0.49	0.26	0.42	0.28	0.28	0.19	0.28
p value	0.98	0.05	0.74	0.20	0.47	0.41	0.86
# obs.	213	213	213	135	135	135	135
# clusters	183	183	183	125	125	125	125
Only non-missing deal values	N	N	N	Y	Y	Y	Y

DOWN vs STAY R1000	# deals per acquirer	# cash	# diversifying	# over2% +div	# over3% +div	# over10% +div	# >$50m
Coefficient	-1.46	-0.63	**-1.49***	-0.60	**-0.83****	**-0.49***	**-0.61***
s.e.	0.94	0.42	0.82	0.38	0.36	0.28	0.35
p value	0.12	0.14	0.07	0.12	0.03	0.08	0.09
# obs.	120	120	120	66	66	66	66
# clusters	106	106	106	58	58	58	58
Only non-missing deal values	N	N	N	Y	Y	Y	Y

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000.All columns are estimated via local linear regression with a triangle kernel and year fixed effects on a bandwidth of 100 observations on each side of the cuttoff. Data from all years are pooled; the sample period is index reconstitutions from 2002-2006. Standard errors are clustered at the firm level. Data is from Russell Indexes and SDC Platinum. # means number of deals. # cash refers to deals with all cash payment. # diversifying means number of deals where the primary SIC code of the acquirer differs from that of the target at the 3 digit SIC level. # over X% + div refers to the number of deals that are both diversifying and for a value exceeding x% of acquirer assets. #>$50m is the number of deals with a transaction value over USD $50 million. Coefficients significant at ten percent or below are in bold; *p* values are displayed in the table.

Table 12 - Voting at Shareholder Meetings

R2000 vs R1000	# of failed shareholder props.	# of failed management props.	Pass rate of management props.	Avg. votes for shareholder props.
Coefficient	**-0.22*** **	**0.06***	**-3.15***	13.7%
s.e.	0.07	0.03	1.8%	12.6%
p value	0.00	0.08	0.09	0.28
# obs.	1983	1983	1845	508
# clusters	1007	1007	986	254

STAY R2000 vs UP	# of failed shareholder props.	# of failed management props.	Pass rate of management props.	Avg. votes for shareholder props.
Coefficient	-0.14	0.10	-4.8%	12.0%
s.e.	0.15	0.07	3.1%	28.7%
p value	0.34	0.17	0.13	0.68
# obs.	791	791	768	81
# clusters	492	492	484	56

DOWN vs STAY R1000	# of failed shareholder props.	# of failed management props.	Pass rate of management props.	Avg. votes for shareholder props.
Coefficient	-0.18	0.00	0.4%	-0.2%
s.e.	0.12	0.05	3.7%	0.5%
p value	0.15	0.94	0.92	0.68
# obs.	1186	1186	1072	426
# clusters	611	611	589	214

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff, averaged across the two years after Index reconstitution. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000. All columns are estimated by OLS with a separate cubic polynomial in the running variable on either side of the cutoff and year fixed effects on a bandwidth of 800 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on index reconstitutions from June 2002 to 2006 inclusive. Standard errors are clustered by firm. Data is from Russell Indexes and RiskMetrics. All dependent variables are from shareholder meetings that occur between Russell index reconstitution dates (end of June), and are averaged over the two years subsequent to each June reconstitution. Number of failed shareholder (management) proposals is the average number of proposals per Annual General Meeting that fail to pass and are sponsored by shareholders (management). Pass rate of management proposals is the number of management proposals that pass divided by the total number of proposals recorded in the Riskmetrics data for each meeting. Average votes for shareholder proposals is the votes_for variable in the RiskMetrics data taken only for proposals submitted by shareholders (Irrc issue codes 2000 or over) and averaged across all shareholders proposals at each meeting. Coefficients significant at ten percent or below are in bold; *p* values are displayed in the table.

Table 13 - CEO Compensation (Robustness table)	Main local linear specification	With firm and CEO controls	With controls for Δ in rank and market cap.	With Industry fixed effects	Removing year fixed effects	Winsorized at 10%	Bandwidth of 75	Bandwidth of 150	5th degree poly. with large bandwidth & controls	Imbens & Kalyanaraman (2012) optimal bandwidth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CEO leaves firm within 2 years										
Coefficient	**17.7%****	**18.5%***	**21.8%****	**16.6%***	**17.1%****	**17.7%****	**19.6%***	**12.7%****	**30.9%****	**15.9%****
s.e.	8.7%	10.7%	9.1%	8.8%	8.9%	8.7%	10.5%	6.6%	14.0%	7.4%
p-value	0.04	0.08	0.02	0.06	0.05	0.04	0.06	0.05	0.03	0.03
# obs.	397	343	298	397	397	397	306	582	1609	493
# clusters	290	251	240	290	290	290	233	383	721	339
Bandwidth	100	100	100	100	100	100	75	150	800	126
Variable/ Fixed Compensation										
Coefficient	**1.74****	**2.19*****	**1.87***	1.32	**1.80****	**1.04***	**2.20****	0.55	2.39	0.72
s.e.	0.88	0.88	1.06	0.86	0.92	0.58	1.12	0.65	1.60	0.67
p-value	0.05	0.01	0.08	0.13	0.05	0.08	0.05	0.40	0.14	0.28
# obs.	376	336	279	376	376	376	287	551	1579	525
# clusters	275	245	222	275	275	275	220	366	713	354
Bandwidth	100	100	100	100	100	100	75	150	800	140
Option pay share										
Coefficient	**16.7%***	**16.4%***	**19.3%****	**14.6%***	**14.5%***	**14.9%***	**22.6%****	**11.1%***	**23.2%***	**11.7%***
s.e.	8.9%	9.7%	9.0%	8.5%	8.9%	8.2%	11.4%	6.4%	12.6%	6.7%
p-value	0.06	0.09	0.03	0.09	0.10	0.07	0.05	0.09	0.07	0.08
# obs.	378	341	280	378	378	378	289	554	1602	528
# clusters	276	249	223	276	276	276	221	368	719	356
Bandwidth	100	100	100	100	100	100	75	150	800	142
Salary pay share										
Coefficient	**-10.9%***	**-11.5%***	**-10.4%***	-9.3%	**-11.5%***	-6.4%	**-13.1%***	-5.6%	**-13.8%***	-7.4%
s.e.	6.0%	6.2%	6.3%	6.0%	6.0%	4.1%	7.8%	4.2%	7.5%	4.7%
p-value	0.07	0.07	0.10	0.12	0.06	0.12	0.09	0.19	0.07	0.12
# obs.	378	341	280	378	378	378	289	554	1602	481
# clusters	276	249	223	276	276	276	221	368	719	329
Bandwidth	100	100	100	100	100	100	75	150	800	129
Restricted stock pay share										
Coefficient	**-9.6%****	**-10.0%****	**-8.8%***	**-10.2%****	**-8.6%***	**-5.1%***	**-9.8%***	**-8.8%*****	-5.9%	**-8.7%*****
s.e.	4.5%	4.6%	5.1%	4.6%	4.7%	2.7%	5.4%	3.5%	5.6%	3.5%
p-value	0.03	0.03	0.09	0.03	0.07	0.06	0.07	0.01	0.29	0.01
# obs.	378	341	280	378	378	378	289	554	1602	537
# clusters	276	249	223	276	276	276	221	368	719	360
Bandwidth	100	100	100	100	100	100	75	150	800	144

This table displays alternative specifications of the main results for the sample composed of firms entering the Russell 1000 from the Russell 2000 and firms remaining in the Russell 2000 in a 100 firm bandwidth on either side of the cutoff. Column 1 reproduces the main specification: a locally linear regression with year fixed effcts and a triangle kernel. Column 2 adds the following controls: CEO age, CEO tenure, firm operating income before depreciation (oibdp), firm sales (sale), total assets (at), market value of the firm (at-seq-pstock+txditc+shrout*price), 1 year sales growth, 1 year asset growth, 1 year market value growth, and number of employees(empl). Column 3 controls for the change in both the firm's market capitalization (source: Russell Indexes) and Russell index ranking over the preceding year. Column 4 adds SIC division fixed effects and column 5 removes year fixed effects from the main specification. Column 6 winsorizes both tails at 10% to test for sensitivity to outliers (the first variable is an indicator so this has no effect). Column 7 reduces the bandwidth to 75 observations, below which ststistical power is limited, and column 8 extends the bandwidth to 150 observations. Column 9 estimates the RDD with a fifth degree polynomial specification and bandwidth of 800 observations on each side of the cutoff and the controls in columns 2 and 3 (including controls becomes more important the larger the bandwidth). Finally Column 10 runs the local linear specification using the optimal bandwidth chosen by Imbens and Kalyanaraman's (2012) algorithm, capped at 200. All columns except for column 9 are estimated by local linear regression.

Table 14 - Accounting variables (Robustness table)	Main local linear specification	With firm controls	With controls for Δ in rank and market cap.	With Industry fixed effects	Removing year fixed effects	Winsorized at 10%	Bandwidth of 50	Bandwidth of 150	Flexible poly. with large bandwidth & controls	Imbens & Kalyanaraman (2012) optimal bandwidth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Capex/Assets										
Coefficient	**-2.04****	**-1.79****	**-1.92***	**-1.32***	**-2.08****	**-1.50****	**-3.46***	**-1.18***	**-1.72***	**-1.76***
s.e.	0.97	0.92	1.11	0.73	0.99	0.66	1.95	0.69	0.95	1.01
p -value	0.04	0.05	0.08	0.07	0.04	0.02	0.08	0.09	0.07	0.08
# obs.	877	868	669	877	877	877	440	1304	2613	740
# clusters	521	516	422	521	521	521	323	661	1152	490
Bandwidth	100	100	100	100	100	100	50	150	800	86

This table displays alternative specifications for Capital Expenditure. The sample is composed of all firms in either index and within the bandwidth for capital expenditures, and firms entering the Russell 1000 from the Russell 2000 and firms remaining in the Russell 2000 for net change in short term debt. Column 1 reproduces the main specification: a locally linear regression with year fixed effcts and a triangle kernel. Column 2 adds the following controls: firm sales (sale), market value of the firm (at-seq-pstock+ txditc+ shrout*price), 1 year sales growth, 1 year asset growth, 1 year market value growth, and number of employees(empl). Column 3 controls for the change in both the firm's market capitalization (source: Russell Indexes) and Russell index ranking over the preceding year. Column 4 adds SIC division fixed effects and column 5 removes year fixed effects from the main specification. Column 6 winsorizes both tails at 10% to test for sensitivity to outliers. Column 7 reduces the bandwidth to 50 observations, below which stastistical power is limited, and column 8 extends the bandwidth to 150 observations. Column 9 estimates the RDD with a flexible polynomial specification (3rd degree for capital expenditures, 4th for change in short term debt) and a bandwidth of 800 observations on each side of the cutoff and the controls in columns 2 and 3 (including controls becomes more important the larger the bandwidth). Finally Column 10 runs the local linear specification using the optimal bandwidth chosen by Imbens and Kalyanaraman's (2012) algorithm, capped at 200. All columns except for column 9 are estimated by local linear regression. All variables are averaged over the two years subsequent to each June reconstitution. Capital Expenditures is capx/at; Net change in short term debt is dlcch/at lagged 1 year.

Table 15 - Voting at Shareholder Meetings (Robustness table)

R2000 vs R1000	# of failed shareholder props.	# of failed management props.	Pass rate of management props.	Avg. votes for shareholder props.
Coefficient	**-0.16***	0.07	-4.4%	14.9%
s.e.	0.09	0.05	2.7%	17.2%
p value	0.07	0.12	0.11	0.39
# obs.	349	349	326	61
# clusters	244	244	236	40

STAY R2000 vs UP	# of failed shareholder props.	# of failed management props.	Pass rate of management props.	Avg. votes for shareholder props.
Coefficient	-0.18	0.07	-3.7%	19.0%
s.e.	0.12	0.06	2.6%	26.1%
p value	0.13	0.24	0.16	0.47
# obs.	202	202	190	32
# clusters	155	155	148	24

DOWN vs STAY R1000	# of failed shareholder props.	# of failed management props.	Pass rate of management props.	Avg. votes for shareholder props.
Coefficient	-0.17	0.03	-4.3%	0.0%
s.e.	0.13	0.07	5.9%	0.1%
p value	0.20	0.60	0.47	0.89
# obs.	136	136	126	27
# clusters	117	117	111	24

Coefficients are Fuzzy RDD estimates of the difference at the cutoff of the regression functions to the left and to the right of the cutoff between the Russell 1000 (R1000) and Russell 2000 (R2000) equity indices. Coefficients should be interpreted as the effect on the dependent variable of being assigned to the R1000 (of larger firms) instead of the R2000 among firms close to the cutoff, averaged across the two years after Index reconstitution. The first panel, labelled R1000 vs R2000, compares all firms in the R1000 and within the bandwidth against all firms in the R2000 and in the bandwidth. The second panel compares firms within the bandwidth that move up into the R1000 to those that remain in the R2000. The third panel compares firms in the bandwidth that move down into the R2000 with those that remain in the R1000. All columns are estimated via local linear regression with a triangle kernel, and year fixed effects on a bandwidth of 150 observations on each side of the cuttoff. Data from all years are pooled; the sample period is centered on index reconstitutions from June 2002 to 2006 inclusive. Standard errors are clustered by firm. Data is from Russell Indexes and RiskMetrics. All dependent variables are from shareholder meetings that occur between Russell index reconstitution dates (end of June), and are averaged over the two years subsequent to each June reconstitution. Number of failed shareholder (management) proposals is the average number of proposals per Annual General Meeting that fail to pass and are sponsored by shareholders (management). Pass rate of management proposals is the number of management proposals that pass divided by the total number of proposals recorded in the Riskmetrics data for each meeting. Average votes for shareholder proposals is the votes_for variable in the RiskMetrics data taken only for proposals submitted by shareholders (Irrc issue codes 2000 or over) and averaged across all shareholders proposals at each meeting. Coefficients significant at ten percent or below are in bold; p values are displayed in the table.

Chapter 2
How do CEOs see their Roles? Management Philosophies and Styles in Family and non-Family firms*

Joint with ANTOINETTE SCHOAR†

1 Introduction

Most companies start out as family firms, with only a small fraction eventually becoming widely held firms with professional management and no significant family involvement. The literature suggests that family firms differ in important ways from non-family firms: Starting with the seminal paper by La Porta et al. (1999) these studies document that family firms on average tend to be smaller than nonfamily firms, have weaker performance, and worse governance structures.[1] In addition, family firms are reportedly slower to adopt managerial best practices than non-family firms (see for example Bloom et al., 2012a). While this literature suggests that differences in the performance of family firms are associated with the involvement of family members in the top management (and control) of the company, we know very little about the types of CEOs that select into these different leadership positions.

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[1] See in particular Morck, Stangeland, and Yeung (2000); Claessens et al. (2002); Faccio and Lang (2002); Anderson and Reeb (2003); Villalonga and Amit (2006); Bertrand and Schoar (2006); and Bennedsen et al. (2007).

In this paper we show that CEOs' business philosophies, their management styles and backgrounds vary systematically with the control rights and the influence that the family has on the business. We also document that the simple dichotomy between family and non-family firms which is often used in the literature masks much more complex dynamics of how leadership structures differ across firms.

Our data come from a unique survey of over 800 CEOs of the largest public and private firms in 22 emerging market countries, which we undertook jointly with the World Bank.[2] These rich, CEO level data provide insight into the heterogeneity in CEO beliefs, their objectives, and the way they manage and organize the firm across countries and industries.

We first show that firms broadly fall into four distinct categories which are directly associated with the characteristics of their CEOs: (1) firms run by the original founder, (2) family firms with a (non-founder) family member as CEO (referred to as a "related CEO" henceforth), (3) family firms with a professional CEO, and (4) non-family firms run by professional CEOs. This CEO type classification explains a substantial fraction of the variation in CEO survey responses, and is complementary (and of comparable importance to) the fraction explained by country fixed effects.[3]

We then show that CEOs' management styles and philosophies vary with the involvement of family members in the firm: In firms where the founder or the family owners are more involved in the management and control, the CEO tends to run a more hierarchical management structure, places less weight on protecting minority shareholder rights, more weight on protecting stakeholders such as workers, and feels most accountable to banks as outside investors. In addition, these CEOs see their role as maintaining the status quo rather than bringing about change. In contrast, professional CEOs of widely held firms are at the other end of the spectrum and display a management philosophy and style that resembles the textbook view of a shareholder-value-maximizing CEO. In the following we discuss our findings in more detail.

Several overarching themes emerge from the data. First, **founder CEOs** are more likely to have much higher cash flow rights and control rights within their firms than all other types of CEOs, especially compared to professional CEOs. These CEOs are also more likely to be on their company Board, to serve as the Chairman of the Board, and to name the

[2] The countries are: Argentina, Brazil, Chile, Colombia, Ecuador, Peru, Venezuela [S. America]; Costa Rica, El Salvador, Guatemala, Mexico [C. America]; Ghana, Kenya, Nigeria, South Africa, Zimbabwe [Sub-Saharan Africa]; Hong Kong (China), India, Malaysia, Singapore [E. Asia]; Egypt, and Turkey.

[3] The average adjusted R^2 of all the regressions in the paper rises from 9% when only SIC and country fixed effects are included, to 15.5% when the CEO type variables are added. Moreover, when only CEO type and SIC fixed effects are included the average adjusted R^2 is 10%.

directors. This centralization of control seems to go hand in hand with a more hierarchical management style and organization. When compared to the other CEO types, founder CEOs have the fewest number of managers reporting directly to them: 45% have fewer than 5, in contrast to the 80% of professional CEOs that have over 5 managers reporting to them, and this difference holds when controlling for industry, firm size and country among other dimensions. Furthermore, founder CEOs are more likely than professional CEOs to see their main task as being to supervise and monitor decisions, so they place less weight on selecting and appraising managers.

Founder CEOs' relative insulation from outside governance seems to have implications for their business philosophy and view of governance. Compared to other CEOs, founders are less likely to report that they feel accountable to shareholders or to consult large shareholders before major investment decisions. By contrast, they are more likely than professional CEOs to consult banks before major investment decisions and to feel accountable to banks. This suggests that founders retain most of the de facto control within the firm, and that the main constraints they face are from third parties such as their banks, rather than from shareholders. In addition, founder CEOs show much less concern for shareholder value: they are 22% more likely than professional CEOs to answer that they would prioritize stable employment over maintaining dividends for shareholders. While this reveals that founder CEOs place less emphasis on protecting value for (minority) shareholders, it also suggests that they have a broader set of stakeholders in mind – since founders tend to be large shareholders in their firms, they themselves will be affected by forgoing dividends. Additionally, founder CEOs are 26% more likely than professional CEOs to see their leadership role as maintaining existing strategies and values, rather than bringing change to the firm; they may have crafted the strategy themselves.

A different portrait emerges of **professional CEOs of *non-family* firms**, which appear to represent the other end of the spectrum of leadership styles and philosophies. Although they are significantly less likely to own equity and to be on the Board than related CEOs, they are just as likely to be Chairman of the Board, indicating relatively high internal control rights. Interestingly, professional CEOs view their role as agents of change for their organizations rather than to maintain traditions and values, and they appear to have sufficient power to do so. Of all CEO types, they are most likely to replace upper-level managers in their first two years as CEO.

Professional CEOs of *non-family firms* are around 15% less likely than professional CEOs of *family firms* and related CEOs to view family relationships as important to successful business, and report that the founders of their firms are less likely to have a role in

naming directors, or advising on major investment decisions. Unlike founder CEOs (and to a lesser extent the other two CEO types), professional CEOs of non-family firms view large shareholders as more important stakeholders than banks, perhaps reflecting the high preponderance of foreign multinational control of this type of firm. The professional backgrounds of such professional CEOs also reflect a different career path: they are more likely to have been CEOs of other firms and to have held senior positions in finance than founders or related CEOs, and are around 14% less likely than all other CEO types to view specific industry knowledge as one of the two most important factors for success, which is suggestive of a generalist background.

Related CEOs and professional CEOs of family firms fall between these first two CEO types on all dimensions. **Related CEOs** tend to be closer to founder CEOs in their responses, as they maintain a high fraction of cash flow and control rights in the firm they run: they are almost as likely to own equity in the firm as founders (61% more likely for founders, and 52% for related CEOs in comparison to both types of professional CEO) and also to own over 5% of the firm (77% more likely for founders, and 62% for related CEOs). They are also the CEO type most likely to be on the Board of their firm, although they are less likely to be the Chairman of the Board than founder CEOs.

In contrast to founders, and even to professional CEOs of non-family firms, related CEOs appear to be less empowered, since they are often supervised by a powerful founder. Related CEOs are more likely than other CEOs to report that the founder is still involved before major investment decisions, that the founder, rather than the Board, terminated the last CEO, and also that they themselves were appointed by the founder. The active presence of the founder might explain why related CEOs seem to resemble founder CEOs in their approach to governance and business philosophy. Like founder CEOs, related CEOs are more likely to feel accountable to banks and are more likely to involve banks in major investment decisions than professional CEOs of non-family firms. Also like founder CEOs, related CEOs are more likely to favor maintaining the firm's values over bringing change, and are approximately 11% more likely to say they would choose to prioritize maintaining employment over paying dividends. The point estimates on all these dimensions are smaller than those for founder CEOs, but they are consistently and significantly different from the answers of professional CEOs, suggesting a systematic difference in business philosophy. Related CEOs are also more likely than other CEOs to say that family relationships facilitate access to business information, perhaps reflecting their personal experience.

Despite this similarity to founder CEOs in governance and business philosophy, the management style of related CEOs seems to be closer to that of professional CEOs. Related

CEOs are much more likely than founder CEOs to view the selection of senior managers as important, and are less likely to answer that supervision of senior managers is a high priority. When looking at the number of managers reporting directly to the CEO, related CEOs are much more likely than founders to have more than 5 managers report to them, but less likely than professional CEOs (although the last difference is not statistically significant).

Lastly we look at **professional CEOs of *family firms***. Based on their business philosophy, management strategies, and attitudes toward governance, they seem very similar to professional CEOs of non-family firms. They are more likely than founder CEOs to feel accountable to shareholders and to favor shareholder value maximization over maintaining employment. They are also more likely than founders to see their most important task as selecting top talent rather than monitoring managers. Moreover, like professional CEOs of non-family firms, and unlike both founder CEOs and related CEOs, they see their role as bringing about change in the business rather than maintaining established strategies. Finally, the number of managers reporting directly to them is the same as for professional CEOs of non-family firms (i.e., relatively high), reflecting a less hierarchical organization than that of firms run by founders or related CEOs.

However, the ambitions of professional CEOs to effect change may be harder to translate into action within a *family firm*: our results suggest that professional CEOs of family firms have fewer explicit or implicit control rights than other CEOs. Like professional CEOs of non-family firms, they have lower ownership of their firm on average, and are less likely to sit on the Board; they are the CEO type least likely to be the Board Chairman or to name directors. Furthermore, professional CEOs within family firms also appear to have fewer effective control rights: in comparison to professional CEOs of non-family firms they have less scope to replace the top management team when they come into the job. This suggests a discrepancy between the reported ambitions of professional CEOs and how much they are empowered to actually implement them.

Finally, we explore the family backgrounds of the CEOs in our sample. On average the CEOs in our sample grew up in predominantly middle or higher income families, with only 14% describing their parental home as low income. The majority have fathers who were businessmen themselves (59%) or even had paternal grandfathers in business (39%). However, there is a sharp difference between the background of founders and professional CEOs of *non-family* firms on the one hand, and the more privileged background of related CEOs and professional CEOs of *family firms* on the other. In comparison to the other CEO types, founder and professional CEOs of non-family firms are (i) more likely to come

from low income families, and are (ii) less likely to have had fathers who were business managers. In contrast, family firms tend to hire CEOs that come from a more privileged background, or to promote their own descendants into these positions (who by definition come from higher income classes). These results suggest that family firms tend to hire people through their social networks, but that positions as founders and as CEOs of non-family firms provide opportunities for upward mobility in these economies.

This paper contributes to both the literature on family firms, and the literature on the impact of CEOs on firm performance. By examining the heterogeneity in governance arrangements across firms of different types, and how it maps onto CEOs' business philosophies and management styles, this paper aims to shed some light on the determinants of the reported differences in firm performance associated with the CEO and with family firm status. The large sample size and wide regional coverage relative to existing CEO surveys make it possible to remove country and industry-specific variation and to explore the relationship between firm governance and CEO business philosophies and management styles. Interestingly, we show that the CEO/firm level variation accounts for the most robust patterns in the data, rather than traditional country-level variables such as GDP per capita, legal origin, corruption, and property rights, suggesting that the CEO/firm level variables are of first order importance.

The rest of the paper is organized as follows. The related literature is reviewed in Section 2, while Section 3 describes the data, provides summary statistics, and describes the regressions. Section 4 discusses the results. Section 5 examines CEO backgrounds, and Section 6 concludes.

2 Related Literature

A large literature has focused on differences between family and non-family firms, starting with the influential paper by La Porta et al. (1999). These studies document that family firms, on average, tend to be smaller than non-family firms, have lower performance, weaker governance structures, and are often concentrated in older, more regulated industries (e.g. Morck, Stangeland, and Yeung, 2000, Claessens et al., 2000, 2002; Faccio and Lang, 2002; Anderson and Reeb, 2003; Bertrand and Schoar, 2006). Attention has also focused on the importance to firm outcomes of the CEO position (e.g. Bennedsen et al., 2010, 2012) and on the individual characteristics and styles of CEOs (e.g. Bertrand and Schoar, 2003; Malmendier and Tate, 2008; Schoar and Zuo, 2011).

At the intersection of these literatures, a number of papers have studied the performance implications of leadership transitions within family firms: the reported lower average rates of return and stock market valuation of family firms seem to be associated with the passing of control from the founder to the heirs (e.g. Pérez-González, 2006; Bloom and Van Reenen, 2007; Bertrand et al., 2008).[4] Furthermore, some studies have found that when management control within family firms is transferred to professional CEOs rather than a descendant of the founder, the decrease in performance is less pronounced (see the important contributions of Villalonga and Amit, 2006 and Bennedsen et al., 2007).

While survey-based data such as that used in this paper have some well-known limitations, they also provide a window into the beliefs, attitudes and governance environments of CEOs that are otherwise entirely inaccessible to researchers (see Graham and Harvey, 2001, Brav et al., 2005, and Graham, Harvey, and Puri, 2011, for short discussions). This is especially true for CEOs of leading firms in developing countries, which are often private. As such, our results are complementary to some of the recent work on differences in management practices across firms and countries (particularly Bloom and Van Reenen, 2007 and Bloom et al., 2012a), and also with recent work by Bandiera et al. (2013) on the time use of CEOs. The latter paper identifies two distinct styles of time use, and reports that the least productive of the two styles is associated with family CEOs in their sample of Indian manufacturing firms. They also find systematic differences between the time use of family CEOs, and that of professional managers, and that family CEOs work shorter hours and are more likely to shirk when temptation arises. In our sample it is founder CEOs that are more likely to report working the most (over 60 hours per week), and we find some weak evidence (in unreported regressions) that related CEOs are more likely to work fewer hours.

The results in this paper are directly relevant to research on the unique role of founder CEOs (Adams et al., 2005; Anderson et al., 2009), to the links between family firms and the political system (Morck and Yeung, 2004), and to the increasingly important issue of family management transitions as large cohorts of family firms mature, as highlighted in Tsoutsoura (2013). Relatedly, the patterns in our data point to many of the problems that firms face when evolving from a founder-run firm into either a more established family firm, or into a non-family firm.

It is possible that certain problems may be imprinted into the structure and composition

[4] A few studies have argued that family firms, in fact, perform better than non-family firms: Khanna and Palepu (2000), Anderson and Reeb (2003), Sraer and Thesmar (2007), and Mehrotra et al. (2011). Moreover, firms led by founder CEOs appear to also perform better (Villalonga and Amit, 2006; Fahlenbrach, 2009), although Bloom et al. (2012a) report lower management practice scores.

of the organization from the beginning (Baron, Hannan, and Burton, 1999), since founder-led firms appear to concentrate implicit and explicit power in the hands of the founder CEO. Such an organizational structure might perform well when the firm is run by an exceptional leader – which founder CEOs often are. However, such a structure is likely to be detrimental when a family successor is less exceptional (which will occur if there is reversion to the mean in ability over generations), or when a leadership transition to a change-oriented professional CEO is triggered by a change in the skills required of the CEO (as in Eisfeldt and Kuhnen, 2013). Kaplan et al. (2012) report that variation in the abilities of (professional) CEOs for buyout and VC-backed firms is primarily explained by two factors: one approximating general ability, and another that contrasts communication and interpersonal with execution skills. They report that the latter skills are valuable only in some settings, providing support for the idea that CEO abilities have to match the changing needs of their firms. This fits well with our evidence that there is substantial heterogeneity in CEO types, as measured by their governance environment, styles and business philosophies.

3 Data and Empirical Strategy

Our survey covers leading CEOs in 24 emerging markets and was conducted in the first half of 2007 in association with the World Bank and the IFC. Questionnaires were sent to the CEOs or Managing Directors of the largest 100 companies in each country. These firms were selected using D&B International, Amadeus, and OneSource databases, stock market information, World Bank country directories, and local lists of incorporation; for firms in business groups we contacted the group holding company CEO rather than subsidiary CEOs.

The final survey contained eight sections: company information, personal information, educational background, prior work experience, the CEO's business approach, family background, country culture, and company structure. The survey is in the appendix. We ran a pilot for Australia and South Africa in 2006, which served to refine the survey instrument and implementation. A team of MBA students conducted phone interviews with CEOs over a four-month period. All the firms in our sample were contacted by one of the callers to set up a time for a phone interview with the CEO. In almost all cases we reached the Assistant to the CEO. If the CEO was not available for a phone conversation, we asked the Assistant to give the CEO a copy of our survey to fill out, or to forward an online link to the survey. Those CEOs that did not answer were then sent a reminder email or fax

ten days after the initial contact, and then subsequently called by a survey team of MBA students up to a maximum of three times. Survey responses relating to firms' industries (at the two-digit SIC code level) and ownership of the firm by multinationals and the Government were verified and augmented with information obtained from firms' websites and annual reports. Additionally, public listing of firms was obtained from OneSource and confirmed on firm websites and Bloomberg where possible; firms were assumed to be private if no listing information could be found.

We keep observations from countries where we have at least 15 completed surveys, so our final sample is composed of 823 CEOs from 22 countries: eleven are in Latin America (covering virtually all the continent's GDP), six are in Africa, and the remainder in Asia (see the appendix for observations by country).[5] This is a large sample relative to the managerial surveys in the finance literature.[6] The average response rate is 37.4% of the top 100 CEOs in 22 countries and has no correlation with GDP (the Spearman's rank correlation is -0.15 with a p-value of 0.51).[7] Our response rate compares favorably to those in other senior management survey studies that range from 9% (e.g., Graham and Harvey, 2001) to approximately 16% (e.g., Brav et al., 2005), and is large enough to mitigate concern about potential response biases.

3.1 Classifying CEOs into types

We group CEOs into four mutually exclusive categories or types based on the responses to our survey and information about the firm:

(i) Founder CEOs (12.6% of the sample)

(ii) Related CEOs (18%)

(iii) Professional CEOs of family firms (21%)

(iv) Professional CEOs of non-family firms (48.4%)

[5] Australia and Romania were dropped because we obtained less than 10 observations from each.

[6] Recent papers using surveys in the finance literature include Graham and Harvey (2001), Brav, Graham, Harvey, and Michaely (2005), and Graham, Harvey, and Puri (2011).

[7] For publicly listed firms for which a comparison sample is available from Bureau van Dijk's Osiris database, one-tailed t-tests (that do not assume equal variances) cannot reject equality of mean sales between the surveyed firms and the comparison sample. However, this is a very partial test because less than a quarter of our sample is both publicly listed and from a country that has at least 50 firms in the comparison sample available to calculate the mean.

Founder CEOs (founders henceforth) are identified from a direct question as to whether the CEO is the founder of the firm. We classify a CEO as a **related CEO** if he or she either answers in the survey that s/he is a relative either of the founder or of shareholders who own at least twenty percent of the firm. To classify a CEO as a **professional CEO** we require him or her to be neither the founder, nor related to the founder, and that his or her family does not own more than 20% of the firm's equity. We also separate firms into either family or non-family firms; founder-led and related CEO-led firms are classified as family firms. Professional CEOs are classified as leading **family firms** if the founder or his family is one of the firm's three largest shareholders. If this is not the case, we classify them as professional CEOs of **non-family firms**. This implies that we may be mistakenly classifying family firms as non-family if either the controlling family is unrelated to the founder, or if the CEO himself – but not his or her family – is the majority owner of the firm but is not the founder. This would reduce our ability to detect differences between family and non-family firms, leading to attenuation bias in our results because our baseline category for the analysis that follows is professional CEOs of non-family firms (approximately half of the sample).

Two other papers use similar classification schemes for CEO types: Anderson and Reeb (2003) and Sraer and Thesmar (2007).[8] In comparison to both papers, our sample has more professional CEOs (of both family and non-family firms) and fewer founder-run firms. The firms in our sample are spread over a wide array of two-digit SIC code industries, but the top ten industries account for 60% of firms in our sample, while the top twenty account for 82%. The top five two-digit industries are: (1) food and kindred products; (2) chemicals and allied products; (3) depository institutions (i.e., banks); (4) general building contractors; and (5) electric, gas, and sanitary services.

3.2 Summary Statistics

Panel A of Table 1 suggests that company characteristics covary directly with the type of CEO leading the firm. In unreported regressions we find that founder and related CEO firms are significantly smaller than non-family firms in terms of (survey-reported) sales, in line with the literature, which reports that family firms are smaller on average[9] and may

[8] Bloom et al. (2012a) also makes use of a similar classification, but focuses on the organization's management practices rather than those of the CEO with regard to his/her top management team. Burkart et al. (2003) model managerial succession as the founder simultaneously choosing both how much equity to sell and, if she retains control, whether to appoint a related or a professional CEO. The choices faced by the founder in their model match the categories used in this paper.

[9] For example, Anderson and Reeb (2003), in a sample of US S&P 500 members, find that family firms are smaller than non-family firms. Villalonga and Amit (2006) find that family firms are smaller, but

be willing to forgo growth to maintain control. The non-family firms in the sample are larger: both in terms of mean and median sales, we find that non-family firms are at least double the size of founder-run firms and are substantially larger than the remaining two firm types on average. Drawing data from Bloomberg for the subsample of firms that are publicly listed we find a similar pattern for mean revenue, suggesting that the self-reported sales data are broadly accurate. There is no difference in means for market capitalization in the subsample of listed firms. In addition, founder-run firms are substantially younger on average than all other types of firms, while non-family firms are more likely to be controlled by multinational parents (38%) than family firms (5% to 14%). Firms run by related CEOs and non-family firms are both more likely than the other two firm types to be publicly listed, either domestically or on a foreign exchange. Finally, the equity ownership of the largest three equity holders is considered in detail in Panel A of Table 3, and we are slightly less likely to see family firms with a professional CEO in English legal origin countries.

In short, between younger, smaller, founder-run firms at one extreme, and larger, often multinational controlled non-family firms at the other, we have family firms run by either related or professional CEOs, which are harder to distinguish along the dimensions in the table.

TABLE 1 ABOUT HERE

Panel B of Table 1 displays individual characteristics by CEO type. In unreported regressions we find that founder CEOs are significantly different from the other types of CEOs on average: they are older, are (naturally) more likely to have been CEO from the start of their time at the firm, have much longer average and median tenures, and are much more likely to own more than 5% of the firm than all the other CEO types. Moreover, founders are less likely to have undergraduate degrees.

Perhaps reflecting an apprenticeship period at the family business, related CEOs are much less likely than all other CEO types to have begun their time at the firm as CEOs, but have both longer tenures and are more likely to own at least 5% of the firm's equity than the two professional CEO types. They are also more likely than all other CEO types to have a degree from a foreign country, potentially because they were groomed for a role at the family firm from an early age, and because of the economic advantages of being related to a successful founder CEO.

not significantly so, and report that they are younger. Sraer and Thesmar (2007) also report that family firms are smaller and slightly younger.

Clear and statistically significant differences emerge between founder and related CEOs on one hand, and the two professional CEO types on the other. Founders and related CEOs are significantly less likely to have held a prior position as a CEO or in a financial field[10], and have much longer tenures (the median is ten years or more in comparison to five or fewer), which is potentially related to their higher likelihood of owning over 5% of their firms.

By contrast, Professional CEOs (at both family and non-family firms) are statistically indistinguishable from each other on all dimensions in the table. Thus, the personal characteristics and professional experience of the two types of professional CEOs are very similar.

The characteristics of the different types of CEOs in our sample match those reported in the literature. As noted earlier, we find that most types of family firms in our sample have lower sales than non-family firms, a standard result, and that founder-run firms are younger. We find that related CEOs themselves are also younger than other CEO types on average, and by approximately the same amount (eight years) as reported by Pérez-González (2006) at the time of CEO transition. Related CEOs in the sample tend to have significantly longer tenures than professional managers, as also noted by Sraer and Thesmar (2007) who report differences of similar magnitudes, and we find CEO personal shareholdings are positively correlated with tenure.

3.3 Description of regressions

A linear probability model (LPM) is used to describe the correlations of the survey responses with explanatory variables. We code the responses as indicator variables which take a value of 1 if the respondent is in agreement with the question or chooses a specific answer from a list of potential answers, and 0 otherwise. We generate a separate indicator variable for each response, e.g., a variable equal to one for all those who choose the answer "Shareholders", and zero otherwise. Each indicator variable is then used as a dependent variable in a LPM regression structured as follows[11]:

[10] Survey respondents were asked to "*list the three positions (business and non-business related, academia, government, military etc.) you held the longest prior to becoming*" CEOs of their firms. These were then classified into CEO positions (CEOs, Executive Chairman, etc.), Board positions (Director, Chairman, etc.), and Financial positions (Finance manager, Comptroller, Treasurer, VP(Finance), etc.) if it was possible to do so.

[11] Two survey responses are estimated by ordered probit because of the ordinal nature of the responses; these are identified in the regression tables.

$$Survey\ response\ _{jics} = \alpha + \Gamma \times CEOtype\ _{j} + \beta \times Controls\ _{ics} + \varepsilon\ _{ijcs}$$

Where j indexes CEO types, i indexes individual CEO-firm pairs, c indexes the country of the firm's headquarters, and s indexes the firm's 2 digit SIC code. The CEO type variables are binary indicators for three CEO types: founders, related CEOs and professional CEOs of family firms. The omitted CEO category is professional CEOs of non-family firms (48.4% of the sample). Taking the question: "Do you feel accountable to shareholders?" as an example, the estimated coefficient on the CEO type indicator ($\hat{\gamma}$) should be interpreted as the additional likelihood of the specific CEO type answering "I feel accountable to shareholders," in comparison to the likelihood of such an answer from the omitted CEO type: *professional* CEOs of *non-family* firms.

The Controls vector contains several sets of variables. Firstly, there are four controls for the overall development of the country of each firm. These are the natural logarithm of GDP per capita from the IMF World Economic Outlook, the average score on the Transparency International Corruption Index over 2003-2007 (the scale runs from 0 (most corrupt) to 10 (least corrupt)), the Property Rights index from the Heritage Foundation's 2004 Index of Economic Freedom (a higher score means more secure property rights), and an indicator variable denoting either English (1) or French (0) legal origin. In the Internet Appendix we reproduce all estimates in the paper using region fixed effects instead of the four country-level controls used as our main specification: results are very similar. The Controls vector also contains (i) a fixed effect for each two-digit SIC industry, (ii) an indicator for whether the firm is publicly listed, (iii) the natural logarithm of firm sales in 2006 (winsorized at 5% and 95% to protect against data coding errors), and (iv) an indicator variable for missing sales information.[12] For observations missing sales (18%) we replace sales with a zero and add a missing sales indicator variable. It is important to control for firm size, since the descriptive statistics show that non-family firms are substantially larger than the other firm types, although including observations with missing sales values instead of dropping them from the sample does not materially affect our results.

Standard errors are clustered at the country level, resulting in twenty two clusters, a number low enough to warrant concern that our standard errors are biased downwards

[12] Firm sales are the best measure of firm size available to us, because earnings measures are distorted by international differences in accounting and stock markets are underdeveloped in many of the countries under consideration (a majority of the sample firms are not publicly listed), ruling out the use of market values. Results are almost identical if we winsorize sales at the 1 or 2% level.

(see Cameron, Gelbach, and Miller (2008) and the references therein). To allay this concern we re-ran all regressions clustering by country*SIC code, an economically meaningful unit which results in around 350 clusters, and we also separately re-ran all regressions using non-parametric block bootstrap by country. Both sets of estimates provide the same or smaller standard errors, so to be conservative we retain the method that produces the largest standard errors: clustering by country.

Finally, to assess the robustness of our results to the regression specification we estimated the regressions using country fixed effects or region fixed effects instead of country controls (in addition to SIC code fixed effects - see the Internet Appendix for regression tables with region fixed effects), and also separately estimated them using a probit specification instead of the LPM. We obtain very similar results, confirming that the results are not sensitive to the inclusion of country/region fixed effects, or to the linearity assumption of the LPM.

4 CEO Governance Structures, Management Styles, and Business Philosophies

4.1 Confirming firm classifications: the Appointment Process of the CEO

To confirm that our CEO type classification is picking up real differences between the categories, we first look at the appointment process of the CEO and how the current CEO sees the influence of the founder and family relationships. To that end we run regressions on a set of survey questions that were not used to generate the categories, but that should predictably vary between the different CEO categories as well as on CEO type indicator variables and our set of control variables. These are reported in **Table** 2, while a visual representation of the differences between CEO types is provided in Figure 1, which graphs the means of regression residuals by CEO type.

The first column focuses on the question: "*Who appointed you as the CEO of «Company-Name»?*" Professional CEOs of family firms are 7% more likely to answer, "*the Founder or his/her relatives*" than the omitted category, professional CEOs at non-family firms. This is to be expected, since the founder probably retired long ago at most non-family firms. Additionally, in column 2 we confirm the status of family firms for those firms run by founders and related CEOs: they are (47 and 77% respectively) more likely to answer

affirmatively to the question "*Were any of your relatives ever employed in an upper-level management position at your firm?*" than professional CEOs of either type.

CEOs were asked, "*Who appoints the board members in your company?*" In column 3 we see that the founder is 17% more likely to appoint directors at family firms with professional CEOs compared to non-family firms. These results lend support to our differentiating professional CEOs by the type of firm they run: family versus non-family firms. Firms run by related CEOs are also 16% more likely to report that directors are appointed by the founder than professional CEOs of non-family firms, which is consistent with our classification of firms as "family firms" when they are run by either related CEOs or what we have called professional CEOs of family firms.

A survey response reported in the Internet Appendix also provides support for the CEO-firm classification scheme. The question is: "*In many countries around the world, mutual support of family members in business transactions is essential for efficient business operations. In your view, how important are family relationships for conducting successful business in your country?*" Both professional CEOs of family firms and related CEOs are 14 to 16% more likely to answer that family background and contacts are important compared to the excluded category, professional CEOs of non-family firms, and also to founder CEOs.

Another dimension that lends support to our classification is whether the founder of the firm is alive, (irrespective of whether he/she is CEO). While we did not ask this explicitly, we can infer it from certain questions, albeit with error. Founders are alive at 51% of related CEO firms, at 44% of family firms with professional CEOs, and at only 23% of non-family firms with professional CEOs. In sum, the results in this section suggest that the four categories of CEO-firm types that we use throughout correspond to real differences in the internal organization of the firms.

FIGURE 1 AND TABLE 2 ABOUT HERE

4.2 Ownership and Governance

We now turn to how governance arrangements vary across firm types. Founder and related CEOs have significantly higher equity ownership, which also translates into substantially more power at the Board level. At the other extreme, professional CEOs that lead family firms seem to have less implicit and explicit control, which might affect their ability

to independently manage their firms. Figure 2 presents a visual representation of the differences between CEO types (after controls) along these dimensions.

In the first column of **Panel A of Table3**, we consider responses to the question: "*As the CEO of «CompanyName» do you hold equity in the firm (stock options)?*" We generate an indicator variable equal to one if CEOs answer in the affirmative, with either "*Yes, I hold more than 5% of the company's stock,*" or "*Yes, I hold less than 5% of the company's stock*". Founders and related CEOs have much higher propensities than both types of professional CEOs to own equity (61% and 52% respectively). In the second column we focus exclusively on CEO responses of "*Yes, I hold more than 5% of the company's stock*". Again, founders and related CEOs have much higher propensities to own over 5% of their firms (77% and 62% respectively), in line with Table 2.

We then regress whether CEOs answer "*Yes, I receive stock and stock options as part of my compensation*" on our standard explanatory variables. Column 3 suggests that founder and related CEOs are less likely to receive stock or options as part of their compensation, perhaps because their holdings are already large on average. However, this effect disappears when we control for firms with a multinational parent corporation in an unreported regression, suggesting that it is driven by firms with multinational parents – which are overwhelmingly non-family firms – choosing equity-linked compensation (for their disproportionately professional CEOs) rather than by differences between CEO types along this dimension.

The number of different types of blockholders present in each firm is obtained from the question: "*Please indicate if any of the three largest equity holders is/are: The founder or relatives of the founder/ Foreign investors/ Foreign corporations/ Domestic corporations/ The government.*" Note that the question does not provide the number of blockholders; if more than one exists in the same category they are counted as a single blockholder. Because the responses are ordinal (0,1,2,3+), ordered probit is used instead of LPM regression for this question. We also obtain the total equity holding (as a percentage) of the largest three shareholders from the following question: "*How concentrated is the ownership of your company? That is, what fraction of equity in your company is held by the three largest shareholders?*"

The marginal effect for the response "two blockholder types" is shown in the table because it is representative of the other responses, but all estimated marginal effects are reported in the Internet Appendix. Interestingly, for family firms run by both related and professional CEOs, we observe a higher number of blockholder types, while the total equity holdings

of the top three shareholders are 12% lower for firms run by related CEO. This suggests equity dispersion may be part of the explanation for why some firms move from founder-run to family firm structures in this sample (instead of to non-family firms), perhaps due to the impact of inheritance taxes or the natural dispersion of ownership from a single founder to more numerous descendants.

FIGURE 2 AND TABLE 3 ABOUT HERE

Panel B of Table 3 reports regression coefficients from additional questions on firm governance. The first column reports coefficients from the question "*Do you sit on your company's Board of directors?*" while the following column focuses on the question "*Are you the Chairman of the Board?*" Founder and related CEO equity ownership is reflected in their high propensities to be on the Board of directors (9% more likely for founders, 13% for related CEOs), and to be Chairman of the Board (23% more likely for founders). The survey also includes the question: "*Who appoints the Board members in your company? Please choose up to three alternatives.*" We generate an indicator variable equal to one if the respondent answers, "*I select most of the Board members*" and this is the dependent variable for the following two regressions in the table. The high average equity ownership of founders and related CEOs is also reflected in their propensity to answer that they select most of the Board members, which is 16% more likely for founders and 7% more likely for related CEOs than for both types of professional CEOs. Thus, the governance structure of founder and related CEO firms appears to differ markedly from those of firms with professional CEOs.

These regressions also suggest that professional CEOs of family firms are relatively disempowered in comparison to the other CEO types. They are the least likely CEO type to be Chairman of the Board (21% less likely), and to name directors (5% less likely). By contrast, non-family firm professional CEOs are as likely as related CEOs to be Chairman of the Board, despite having lower equity holdings on average, as reported in Panel A.

The results also suggest that related CEOs are often monitored by a powerful founder figure. Consider the results in Table 2: related CEOs are the most likely to have been appointed by the founder (over 30% more likely than all other CEO types), and at such firms with a related CEO, founders are over 16% more likely to appoint directors than at non-family firms run by professional CEOs. Moreover, founders are just as likely to appoint directors if a relative or a professional is CEO as they are when they themselves are CEO. Returning to Panel B of Table 3, the previous CEO was more likely to have been terminated by the founder: we report the results of regressions of an indicator variable

equal to one if respondents answer, "*Company founder terminated his appointment*" to the question "*Why did the previous CEO leave «CompanyName»?*" Related CEOs are 10% more likely to answer this than the omitted category, professional CEOs of family firms.

In addition to this increased ability to hire and fire the CEO and name directors, company founders are also more influential in major business decisions at family firms with related CEOs than at firms with other CEO types. We ask CEOs: "*Which of the company's stakeholders are you most likely to involve before deciding to undertake a large-scale investment project, such as the acquisition of a plant or a company? Please select up to two*" and make an indicator variable for when CEOs answer "Founder". Related CEOs are 13% more likely to consult the founder before major investment decisions than other CEO types. All the other answers (except for major shareholders and banks, which are discussed later) show no significant differences between CEO types. [13] Thus, related CEOs appear more likely to be directly supervised by the company founder, and consequently appear to be less empowered than professional CEOs at non-family firms and, naturally, founder CEOs.

4.3 CEO Management Styles

Panel A of Table 4 and **Figure 3** illustrate differences in management approaches between the four types of CEOs concerning their reporting structure, most important tasks, and responsibilities. In comparison to other CEO types, founders appear to disproportionately favor direct management over delegation when asked the question: "*As the CEO of «CompanyName», what do you perceive as your most important operational tasks?*" Founders are 9% more likely to answer that their most important operational task is "*Supervising operational, strategic, and financial planning decisions*" and 14% less likely to answer "*Selecting and appraising other top managers in the company*". Moreover, they design their organizations to have fewer subordinates directly reporting to them than any other CEO types – the most hierarchical structure. They are 15% more likely than all other CEO types to have fewer than five managers reporting directly to them in response to the question "*How many managers in your company report directly to you?*"

Our results are in line with some recent findings for the US. Like Rajan and Wulf (2006) who exclusively examine public firms, we find that professional CEOs in both widely held firms and in family firms manage flatter organizations and have more managers reporting

[13] The other available answers were the parent company CEO, the Government, members of the board, or other top executives in the company

directly to them. By contrast, founder CEOs have the most hierarchical reporting structures. Guadalupe and Wulf (2010) and Bloom et al. (2010, 2012b) provide evidence that increased competition leads firms to decentralize decisions and to improve management practices. Moving the founder out of the CEO position, with an attendant change in internal organization, may be the response of family firms to competitive pressure (or a response to other factors such as the age of the founder). As in Bloom et al. (2012a) we find that founder run firms appear to have the weakest organizational management practices of all firm types in the sample, which may reflect a reliance on idiosyncratic management structures put in place by the founder.

In contrast to founders' autonomy in structuring their management teams, Professional CEOs of family firms appear to have more limited freedom to fire top executives in their initial years as CEO. In response to the question "*How many of the upper-level managers did you replace in the first two years after you took office as the CEO of «Company-Name»?*" we generate an indicator variable equal to zero if the answer was "*None*" and one for the other answers (All/More than half/Less than half). Professional CEOs of family firms are 8% less likely to report dismissing any senior managers in the first two years of their tenure. This suggests constraints on professional CEOs' ability to bring their professional expertise to bear on firms originally structured by the founding CEO, potentially due to organizational imprinting (Baron, Hannan, and Burton, 1999).

Founder CEOs are less likely to report having fired any top executives in their first two years (a largely mechanical result as they are likely to have hired all the executives themselves), but interestingly, related CEOs are also less likely to report firing top managers, albeit only at the 10% level of statistical significance. This again suggests limits on their ability to effect changes to the structure they inherit from the founder.

Finally, professional CEOs of non-family firms appear to have a more generalist focus than the other types of CEO. In response to the question "*Which do you consider to be the most important factors to being a successful CEO in your country?*" they are 12 to 14% less likely than all other CEO types to answer "*specific industry knowledge*".

FIGURE 3 AND TABLE 4 ABOUT HERE

4.4 CEO Business Philosophies

Panel B of Table 4 and **Figure 3** report responses to questions about each CEO's business philosophy and strategic focus. CEOs were asked "*Which of the following alternatives*

best describes your strategic focus for «CompanyName» in the next five years? Please select one: Diversify into new industries/ Expand into international markets/ Strengthen focus in core businesses." Founder CEOs are 17% more likely than other CEO types to express a preference for international expansion over the alternatives, which may reflect the firms they run being at an earlier stage in the life cycle - recall that founder run firms are the youngest firm type in the sample. When asked whether executives should "*Maintain payments to shareholders, even if they must lay off a number of employees*" or should instead choose to "*Maintain stable employment, even if they must reduce payments to shareholders,*" founders (and related CEOs) come out strongly in favor of maintaining stable employment: they are 22% (11%) more likely to do so than the other CEO types. This fits with the result in Sraer and Thesmar (2007) that heir-managed family firms in France smooth the effects of industry sales shocks on labor demand, although here the result is present at founder-run firms also. It is also in line with the idea in Shleifer and Summers (1988) that family control makes possible implicit or relational contracts with a firm's workforce, and Mueller and Phillipon's (2011) argument for the beneficial effect on firms' labor relations of family control.

However, on other dimensions founder CEOs have similar views to related CEOs, and both differ in their responses from professional CEOs of either type. In particular, both founders and related CEOs are more likely to see their leadership role at the firm as "*Guaranteeing the stability of the company's traditions and values*" rather than "*Bringing about changes in the way the company is run,*" unlike professional CEOs of both types. This view is more pronounced for founders (for whom it is 26% more likely versus 14% for related CEOs), which is to be expected from the individual who likely created these traditions and instilled the values. However, it may also be a symptom of the monitoring performed by a powerful founder (as discussed in the ownership and governance section). Related CEOs' freedom of action may be limited by this monitoring, and so they may see their mandate to be administering the firm as they received it from the founder. By contrast, Professional CEOs of family firms, as "outsiders", are more inclined to make changes, which are likely essential to their ability to contribute to the firm and may be why they were initially appointed. Professional CEOs of non-family firms also see their leadership role in terms of making changes rather than maintaining values and traditions.

Turning to CEOs' views on accountability, banks (rather than shareholders) appear to be viewed as the key stakeholders by both founders and – to a lesser extent – related CEOs. In response to the question "*As the CEO of «CompanyName», who do you feel the most accountable to?*" founders and related CEOs are more likely to report feeling accountable to banks than professional CEOs (10% and 6% respectively). Moreover, both CEO types

are more likely to report involving banks before major investment decisions (12% and 8% respectively) in response to the question: "*Which of the company's stakeholders are you most likely to involve before deciding to undertake a large-scale investment project, such as the acquisition of a plant or a company?*" This may reflect the smaller size of their firms (as measured by average sales) and consequently more limited access to capital, or perhaps a closer working relationship.

In contrast to their attitude towards banks, founders – but not related CEOs – are 12% less likely than other CEO types to feel accountable to shareholders.[14] Moreover, they are 18% less likely to report involving large shareholders before major investment decisions than other CEO types. Thus, founder CEOs are particularly likely to view banks, not shareholders in general (or even large shareholders), as their most important external stakeholders, perhaps because founders themselves are large or controlling equity holders. Related CEOs appear to share founder CEOs' concern for banks.

5 CEO Family Ties and origins

5.1 Politics and Family Ties

A final group of survey questions considers CEOs perceptions of the links between politics and firms in their country as a whole (as opposed to their own behavior as CEO), and are reported in the Internet Appendix for brevity. Interpreting such questions involves the difficulty that CEOs respond based on their perceptions of general practice in their countries, which may be less accurate than questions about the CEOs' own behavior. Survey respondents were asked "*How would you describe the relationship between major companies and political parties in your country?*" Related CEOs are 22% more likely to answer "*most business leaders try to maintain close relationships with all political parties and candidates*" (as opposed to only one or none at all) than all other CEO types. They are also 18% less likely than the other CEO types to answer that "*most business leaders do not have close relationships with any political party or candidate.*" That related CEOs in particular should see contacts with politicians across the political spectrum as central to the role of business leader is consistent with the Morck and Yeung (2004) hypothesis

[14] For this regression we control for firms being owned by a parent corporation and by a multinational corporation, as accountability to shareholders will likely be lower if the CEO is insulated from them by a parent corporation in another country. The inclusion of these controls is immaterial to the coefficient estimates of all other regressions in the paper.

that family controlled firms are best suited to the role of counter-parties in rent-seeking games with long-serving officials in Government, due to the prospect of repeated games and the many points of contact between these firms and the Government.

In a similar vein, both professional CEOs of family firms and related CEOs are 14 to 16% more likely to answer that family background and contacts are important for conducting successful business compared to both professional CEOs of non-family firms, and to founders. Moreover, related CEOs are also more likely than other CEOs to say that family relationships facilitate access to business information in their country, in response to the question "*In your view, which of following transactions are most commonly facilitated through family relationships in your country?*" This is unsurprising if, as is probable, their families are wealthier and thus better connected than those of other CEO types. There are no differences between CEO types on any of the other available alternative answers: access to credit, relationship with the Government and regulators, hiring, sale and purchase of assets, or supplier and customer relationships. In sum, the results in this section lend support to the view that family firms (but not founder-run firms) see political interaction and family contacts as a key factor for success.

5.2 The Family Backgrounds of CEOs

The survey also allows us to examine the differences in family backgrounds by CEO type in the sample. We regress responses regarding CEOs' parental income and occupation on CEO type indicator variables and our standard set of control variables (regression coefficients are reported in the Internet Appendix for brevity). The question "*How would you classify the income level of your parents when you were growing up (compared to other families in your country of residence)? Please select one (lower, middle, upper)*" shows that related CEOs are over 16% less likely to have been brought up in a low income household (and are over 30% more likely than all other CEO types to be brought up in a high income household in unreported regressions). Thus, related CEOs appear to have more privileged backgrounds, which is not surprising given that they are related to the founder of a firm that was one of the 100 largest firms in their country at the time of the survey.

Additionally, professional CEOs of family firms are also less likely (7%) to have been brought up in low income households than either founder CEOs or professional CEOs of non-family firms, although unlike related CEOs they are no more likely to be brought up in high income households.

Fathers' occupation is an alternative measure of CEO family background. CEOs were asked "*What is/was your father's main occupation?*" which we classify into three broad occupation categories: business people, blue-collar workers, and professionals. CEOs whose fathers were medical doctors, judges, engineers, teachers, high or middle income Government officials, or other clearly identifiable professionals were grouped into the professional category. Those whose fathers were business owners or managers – whether large (>100 employees) or small – or high or middle income farmers were categorized as being in business, while fathers who were salesmen, clerks, manual workers, or artisans were categorized as blue-collar workers, along with low income farmers or low income Government officials (combinations of the answer to this question and the previous question). Indicator variables for each of these three categories were regressed on the CEO type indicator variables and standard controls.

Related CEOs' fathers, unsurprisingly, are 19% less likely to have been blue-collar workers than the other CEO types' fathers and are approximately 40% more likely to have been in business (in unreported regressions). Further, we see that both founders and related CEOs are respectively 14% and 23% less likely than professional CEOs to have had a professional father (although for different reasons).

We then focus on a smaller grouping of responses to the question "*What is/was your father's main occupation?*" In particular, we look at the likelihood of responding either "*Large business manager (>100 employees)*" or "*Small business manager (<100 employees)*" and group them into an indicator variable for business managers. We do the same for the business owner variable with the responses "*Large business owner (>100 employees)*" or "*Small business owner (<100 employees).*"

Professional CEOs of family firms were 8% more likely to have had a father who was a business *manager* than either professional CEOs of non-family firms or founders. However, there are no statistically significant differences for such CEOs in the likelihood of having fathers who were business *owners*. There is a mechanical effect for related CEOs, while founder CEOs are 17% more likely to have had a father who owned a business at the 10% significance level, which fits with the entrepreneurship literature's finding that a major determinant of self-employment is parental self-employment (e.g., Sørensen, 2007).

Thus, the CEO types appear to coalesce into two groups with regard to family backgrounds: less privileged founders and professional CEOs of non-family firms, in contrast to more privileged related CEOs and professional CEOs of family firms, both of which are more likely to have had fathers who were business managers (and owners in the case

of related CEOs). These results suggest that positions as founders and as CEOs of non-family firms provide opportunities for upward mobility in these countries. In contrast, family firms promote their own descendants into these positions or to hire professional CEOs that come from higher income classes.

The data allows us to examine the family backgrounds of the CEOs in our sample in even greater detail. A large literature focuses on inter-generational social mobility and the resulting income distribution (see Solon, 2002, for a review of the economic literature on inter-generational earnings mobility). CEOs' pay levels and their position atop the income distribution have also generated a large literature (e.g. Piketty and Saez, 2003). We analyze the socio-economic backgrounds of CEOs by examining their fathers' and paternal grandfathers' occupations, which are key proxies for the social status of the CEO's family, and perhaps the most reliable measure available in countries lacking comprehensive longitudinal data series.[15]

The survey also contains a broad measure of CEOs' paternal income when the CEO was growing up (as mentioned earlier), which is an additional measure of their parents' economic status. Parental economic standings are directly relevant to CEOs' social origins, as the inter-generational mobility literature finds them to be a significant source of advantage (e.g. Erikson and Goldthorpe, 2002; Solon, 2002).

TABLE 5 ABOUT HERE

Table 5 shows that the CEOs in this sample overwhelmingly come from middle (52%) or high income (34%) families, suggesting important limits to inter-generational mobility for this selected sample of successful and likely talented managers. However, there are economically important and statistically significant differences between the CEO types. Firstly, and perhaps unsurprisingly, related CEOs are more likely to have grown up in a high income household (63%) and less likely to be from middle income households compared to the other CEO types. More interestingly, founder CEOs and professional CEOs of non-family firms are significantly more likely to have grown up in low income households in comparison to the other two CEO types (related CEOs and professional CEOs of family firms), as noted earlier. These results suggest that the entrance of CEOs either as founders or the heads of non-family firms allows talented individuals from less privileged backgrounds to become involved in the management of the largest firms within a

[15] The sociology literature has a long history of examining inter-generational mobility as measured by occupational categories (e.g., see Erikson and Goldthorpe, 2002, for a sociological perspective). We are aware that by focusing on fathers and grandfathers we exclude the inter-generational linkages due to mothers and grandmothers, but our data make this inevitable.

country. If we believe that there is regression to the mean in managerial abilities, allowing a transition of managerial power to people outside the narrow circle of family members is likely to be a net improvement, since the firm can draw from the full talent distribution in the country.

These results find broad support when we turn to the occupational backgrounds of the CEOs' parents and grandparents. Table 5 also separates the fathers of CEOs into three broad occupation categories: business people, blue-collar workers, and professionals, as described earlier. CEOs whose fathers were medical doctors, judges, engineers, teachers, high or middle income Government officials, or other clearly identifiable professionals were grouped into the professional category. Those whose fathers were business owners or managers – whether large (>100 employees) or small – or high or middle income farmers were categorized as being in business, while fathers who were salesmen, clerks, manual workers or artisans were categorized as blue-collar workers, along with low income farmers or low income Government officials.

We then repeat the same classification for the CEOs' grandfathers. But we also separate out the categories "Government worker" and "farmer" for the paternal grandfathers because of their high frequency and because we have no measure of income for grandfathers.[16]

The fathers of the CEOs in our sample were principally businesspeople (59%), again suggesting it may be harder to make the occupational leap from a non-business family environment to top CEO status in a single generation. In addition, related CEOs are less likely to have a blue-collar father and are (mechanically) more likely to have a father in business (71% are either sons/daughters or grandsons/granddaughters of founders) than all other CEO types. For further discussion of the socio-economic backgrounds of the CEOs in the sample is presented in the Internet Appendix.

6 Conclusion

In this paper we show that firms broadly fall into four distinct categories: (1) firms run by the founder, (2) family firms with a family member as CEO, (3) family firms with a

[16] Long and Ferrie (2007) use a similar four category classification scheme with (much older) British and US census data from 1860 to 1900. These are (1) white collar (professional and technical workers, as well as retail and clerical workers; (2) farm owners; (3) skilled and semi-skilled workers (craftsmen and factory operatives); and (4) unskilled workers (including farm laborers). They note that using more categories alters none of their substantive findings.

professional CEO, and finally (4) non-family firms run by professional CEOs. This categorization holds across geographic regions and may track the stages of a firm's lifecycle. Founder-run firms concentrate cash flow and control rights in the hands of the founder CEO to a large extent, and show a more hierarchical organizational structure, less delegation to other top managers and more direct monitoring of employees. This centralization of power may explain why founder CEOs report feeling less accountable to shareholders than other CEO types, although they are more likely to acknowledge feeling accountable to banks. It also gives them the freedom to adopt a more stakeholder-centric view, as illustrated by their preference for maintaining stable employment at the firm over paying out dividends.

This structure might be beneficial while the founder CEO is in place, since they are often individuals of exceptional talent. However, the existence of such a centralized management structure appears to create distortions once the founder is no longer running the firm, and may be part of the explanation for the reduced performance reported in the literature on transitions from founders to family CEOs. We find that family CEOs broadly mimic the approach of the founder and often still consult the founder for important decisions, which might reflect that they do not have the same business acumen as professional CEOs or the founder. A potential expectations mismatch emerges for professional CEOs leading family firms. These individuals see their role as bringing about change, and they aim to implement a modern business philosophy. However, their ability to implement change seems to be significantly curtailed, since the founder appears to retain control of many major management decisions and also limits the CEO's de facto power. For example, such CEOs likely must take over the existing management team instead of being allowed to hire their own team, in contrast to professional CEOs at non-family firms. Ultimately, such provisions might reduce the decision authority of the CEO position and may make it more difficult to attract a high-quality professional CEO to a family firm.

These results suggest that family firms and widely held firms are different not only in their explicit governance structures, but also in terms of the softer factors that affect management effectiveness, such as the way they set up their operations or their business philosophy. These differences also highlight that the importance and mandate of the CEO position varies based on the type of firm they are hired into, and this internal firm dynamic, in turn, can affect the type and quality of managers that select into each firm type. The results add to our understanding of why we see the differences in performance and managerial outcomes across family and non-family firms that have been documented in the literature. However, much remains to be learned about the internal dynamics of family firms, and how such firms transition into professionally managed, widely held firms.

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8 Figures and Tables

Table 1: Summary statistics

Panel A: Firms	**Whole sample**	Founder CEO	Related CEO	Prof. CEO of family firm	Prof. CEO of non-family firm	*# of obs in calculation*
% of each CEO type		12.6%	18.0%	21.0%	48.4%	*823*
Firm age	**38**	22	42	38	42	*756*
Sales (mean)	**386**	202	288	373	474	*671*
Sales (median)	**116**	42	100	100	148	*671*
Publicly listed firms	**26%**	17%	30%	20%	29%	*823*
% ownership of top 3 equity holders	**78%**	78%	70%	78%	81%	*656*
Firms with a parent company	**45%**	30%	20%	46%	59%	*793*
Firms owned by a multinational	**23%**	10%	5%	14%	38%	*823*
% English legal origin	**32%**	33%	28%	22%	37%	*823*
Panel B: CEOs						
CEO age	**52**	56	50	51	52	*816*
% Men	**98%**	97%	97%	96%	98%	*822*
CEO education:						
Undergrad. %	**91%**	82%	90%	92%	93%	*819*
MBA or grad. degree %	**46%**	35%	46%	50%	48%	*823*
International educ. %	**50%**	41%	62%	46%	50%	*807*
Prior position						
On a Board of Directors	**14%**	19%	15%	14%	12%	*823*
As a CEO	**33%**	21%	22%	41%	37%	*823*
In Financial field	**15%**	6%	8%	20%	18%	*823*
1st job at current firm was as CEO	**42%**	63%	17%	42%	47%	*732*
Tenure (mean)	**8**	15	12	6	5	*738*
CEO owns >5% of firm	**26%**	80%	67%	7%	3%	*766*

Sales are winsorized at 5% and 95%. Sales statistics are in million USD (2006). Firm and CEO ages, as well as tenure statistics, are in years. Firm age is winsorized at 5% to reduce the impact of respondent recall errors (i.e. date of founding versus incorporation versus public listing). Results are virtually identical without winsorizing. See the Internet Appendix for selected summary statistics by region.

Table 2: Confirming firm classifications

	CEO appointed by founder or his/her relatives	Were any of your relatives ever in the upper management of large firms?	Founder names Directors
Founder CEO	0.23***	0.47***	0.16***
	[6.348]	[6.563]	[3.297]
Related CEO	0.31***	0.77***	0.16***
	[5.621]	[18.935]	[3.071]
Prof. CEO of Family firm	0.07***	-0.03	0.17***
	[3.082]	[-1.096]	[6.192]
Country-level controls	Y	Y	Y
Sales & public listing controls	Y	Y	Y
SIC code fixed effects	Y	Y	Y
Observations	802	733	786
Adjusted R2	0.19	0.57	0.11

Regressions of survey responses on a constant, three CEO type indicator variables (the omitted category is professional CEO of a non-family firm), four country-level variables, two-digit SIC code fixed effects, an indicator for whether the firm is publicly listed, the natural logarithm of firm sales in 2006, and an indicator for missing value of firm sales. The country level variables are: the natural logarithm of per capita GDP, the Transparency International corruption index (higher values indicate low corruption), the Heritage Foundation property rights index (higher scores indicate more secure property rights), and an indicator variable for English or French legal origin (1 denotes English). All dependent variables are indicator variables, taking a value of 1 if the respondent is in agreement with the question or answers in the affirmative, and a 0 otherwise. Regressions are estimated using the linear probability model. t-statistics are in parentheses below each coefficient. *, **, and *** denote coefficients significant at 10%, 5%, and 1% respectively. Standard errors are clustered by country. See Internet Appendix for reporting of alternate specifications and more estimated coefficients.

Table 3: Ownership and Governance

Panel A Ownership	Does CEO own equity in firm?	Does CEO own >5% of firm?	Does CEO receive stock/options in compensation?	Number of blockholder types(2)	Equity % of 3 largest shareholders
Founder CEO	0.61***	0.77***	-0.20***	0.07***	-6.93**
	[11.653]	[16.006]	[-3.107]	[3.45]	[-2.295]
Related CEO	0.52***	0.62***	-0.18**	0.10***	-11.58***
	[9.403]	[18.292]	[-2.680]	[5.51]	[-4.216]
Prof. CEO of Family firm	0.07	0.02	-0.10*	0.14***	-5.72*
	[1.425]	[0.840]	[-2.025]	[7.71]	[-1.967]
Country-level controls	Y	Y	Y	Y	Y
Sales & public listing controls	Y	Y	Y	Y	Y
SIC code fixed effects	Y	Y	Y	Y	Y
Observations	821	765	577	750	655
Adjusted R2	0.31	0.54	0.14		0.18

Panel B Governance	Is CEO also on Board?'	Is CEO also Chairman?	Does CEO name most directors?	Was previous CEO terminated by the founder?	Is Founder involved in major investment decisions?
Founder CEO	0.09***	0.23***	0.16***	0.04	0.03
	[3.635]	[3.906]	[3.578]	[1.114]	[0.623]
Related CEO	0.13***	0.08	0.07*	0.10***	0.13**
	[3.445]	[1.410]	[1.772]	[3.490]	[2.750]
Prof. CEO of Family firm	-0.01	-0.21***	-0.05**	0.03**	0.04
	[-0.370]	[-5.377]	[-2.613]	[2.363]	[0.686]
Country-level controls	Y	Y	Y	Y	Y
Sales & public listing controls	Y	Y	Y	Y	Y
SIC code fixed effects	Y	Y	Y	Y	Y
Observations	806	804	786	706	718
Adjusted R2	0.15	0.14	0.06	0.05	0.21

Regressions of survey responses on a constant, three CEO type indicator variables (the omitted category is professional CEO of a non-family firm), four country-level variables, two-digit SIC code fixed effects, an indicator for whether the firm is publicly listed, the natural logarithm of firm sales in 2006, and an indicator for missing value of firm sales. The country level variables are: the natural logarithm of per capita GDP, the Transparency International corruption index (higher values indicate low corruption), the Heritage Foundation property rights index (higher scores indicate more secure property rights), and an indicator variable for English or French legal origin (1 denotes English). All dependent variables are indicator variables, taking a value of 1 if the respondent is in agreement with the question or answers in the affirmative, and a 0 otherwise; regressions are estimated using the linear probability model. The exception to this is the Number of Blockholder Types regression in Panel A, which is estimated via Ordered Probit, and is the predicted value for 2 blockholder types (all predicted values are in the Internet Appendix). t-statistics are in parentheses below each coefficient. *, **, and *** denote coefficients significant at 10%, 5%, and 1% respectively. Standard errors are clustered by country. See Internet Appendix for reporting of alternate specifications and more coefficients.

Table 4: Style and Business Philosophy

Panel A **Style**	Most important task is supervising decisions	Most important task is selecting and appraising managers	CEO has <5 managers reporting directly to him/her	Did CEO replace any top managers in 1st 2yrs as CEO?	Most important factor for success is industry knowledge
Founder CEO	0.09**	-0.14**	0.15***	-0.17***	0.14**
	[2.403]	[-2.351]	[4.08]	[-3.617]	[2.264]
Related CEO	-0.03	-0.02	0.04	-0.09*	0.14**
	[-0.954]	[-0.439]	[1.14]	[-1.827]	[2.812]
Prof. CEO of Family firm	-0.02	-0.06	0.03	-0.08**	0.12**
	[-0.614]	[-1.110]	[1.43]	[-2.269]	[2.151]
Country-level controls	Y	Y	Y	Y	Y
Sales & public listing controls	Y	Y	Y	Y	Y
SIC code fixed effects	Y	Y	Y	Y	Y
Observations	804	804	802	780	821
Adjusted R2	0.05	0.07		0.02	0.14

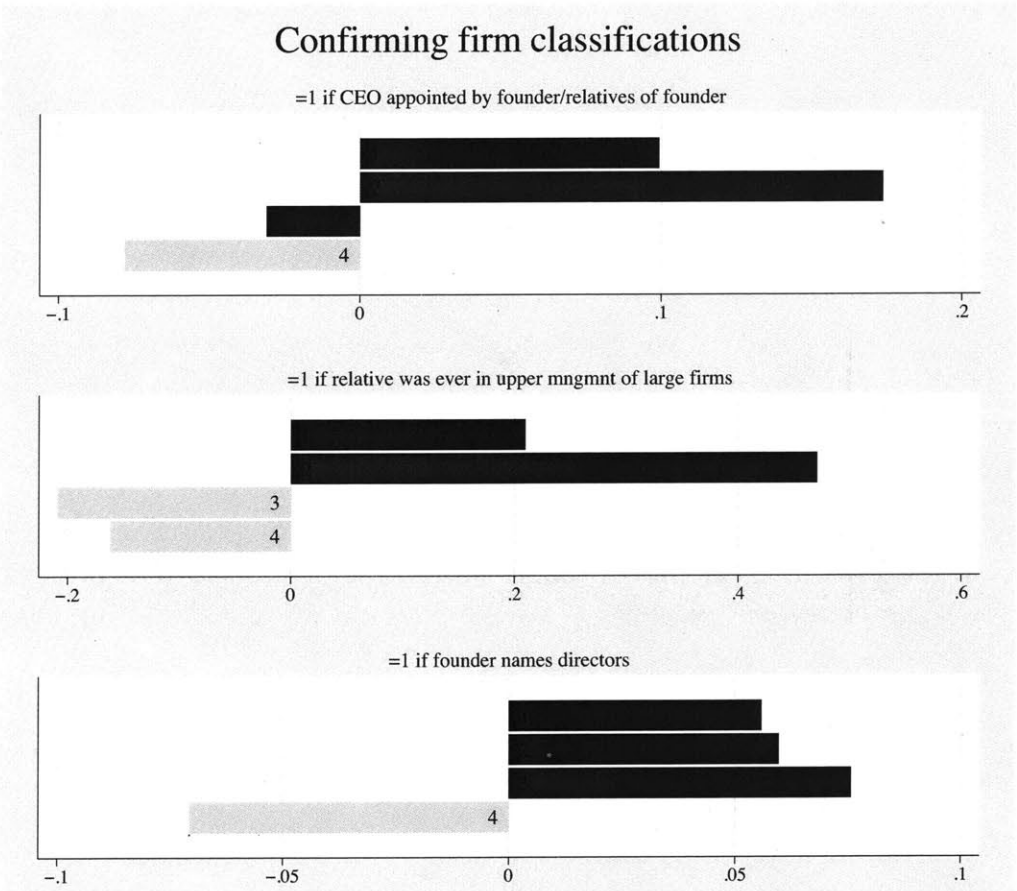
Panel B **Business Philosophy**	CEO management focus is intl expansion	CEO Prioritizes stable employment over dividends	Leadership role is to bring change rather than maintain values	CEO feels accountable to banks	Banks are involved before major investment decisions	CEO feels accountable to shareholders	Large shareholders involved before major investment decisions
Founder CEO	0.17***	0.22***	-0.26***	0.10**	0.12***	-0.12**	-0.18***
	[3.167]	[3.086]	[-4.332]	[2.638]	[3.187]	[-2.518]	[-3.530]
Related CEO	-0.02	0.11**	-0.14***	0.06**	0.08***	-0.02	-0.14*
	[-0.277]	[2.149]	[-3.037]	[2.666]	[3.706]	[-0.345]	[-2.032]
Prof. CEO of Family firm	0.03	-0.04	0.01	-0.00	0.05**	0.02	-0.04
	[0.933]	[-0.765]	[0.138]	[-0.001]	[2.096]	[0.372]	[-1.013]
Country-level controls	Y	Y	Y	Y	Y	Y	Y
Sales & public listing controls	Y	Y	Y	Y	Y	Y	Y
SIC code fixed effects	Y	Y	Y	Y	Y	Y	Y
Observations	807	761	800	808	717	787	717
Adjusted R2	0.05	0.11	0.06	0.09	0.07	0.076	0.04

Regressions of survey responses on a constant, three CEO type indicator variables (the omitted category is professional CEO of a non-family firm), four country-level variables, two-digit SIC code fixed effects, an indicator for whether the firm is publicly listed, the natural logarithm of firm sales in 2006, and an indicator for missing value of firm sales. The country level variables are: the natural logarithm of per capita GDP, the Transparency International corruption index (higher values indicate low corruption), the Heritage Foundation property rights index (higher scores indicate more secure property rights), and an indicator variable for English or French legal origin (1 denotes English). All dependent variables are indicator variables, taking a value of 1 if the respondent is in agreement with the question or answers in the affirmative, and a 0 otherwise; regressions are estimated using the linear probability model. The exception to this is the <5 Managers Reporting directly to the CEO regression in Panel A, which is estimated via Ordered Probit (estimated marginal effects for all responses are in the Internet Appendix; this response is representative). t-statistics are in parentheses below each coefficient. *, **, and *** denote coefficients significant at 10%, 5%, and 1% respectively. Standard errors are clustered by country. See Internet Appendix for reporting of alternate specifications and more estimated coefficients.

Table 5: CEO Parental Income

	Whole sample	Founder CEO	Related CEO	Prof. CEO of family firm	Prof. CEO of non-family firm
CEO's Parents' income					
% low	**14%**	23%	3%	9%	18%
% middling	**52%**	49%	34%	61%	57%
% high	**34%**	28%	63%	30%	25%
Father's occupation					
Bluecollar worker (%)	**15%**	19%	1%	15%	19%
Professional (%)	**26%**	17%	8%	31%	34%
In business (%)	**59%**	65%	92%	54%	46%
Paternal grandfather's occupation					
Bluecollar worker (%)	**11%**	11%	8%	10%	13%
Professional (%)	**13%**	11%	6%	15%	14%
In business (%)	**39%**	39%	65%	38%	29%
In Government (%)	**8%**	4%	5%	9%	9%
Farmer (%)	**29%**	34%	16%	28%	35%

Figure 1



Bars are means of regression residuals after controls (as described in corresponding tables) by the following CEO types: 1: CEO Founder; 2: Related CEO; 3: Professional CEO of a Family Firm; 4: Professional CEO of a Non–Family firm. Bars of darker colors denote significant differences from the baseline category (4) in the corresponding regression tables.

Figure 2

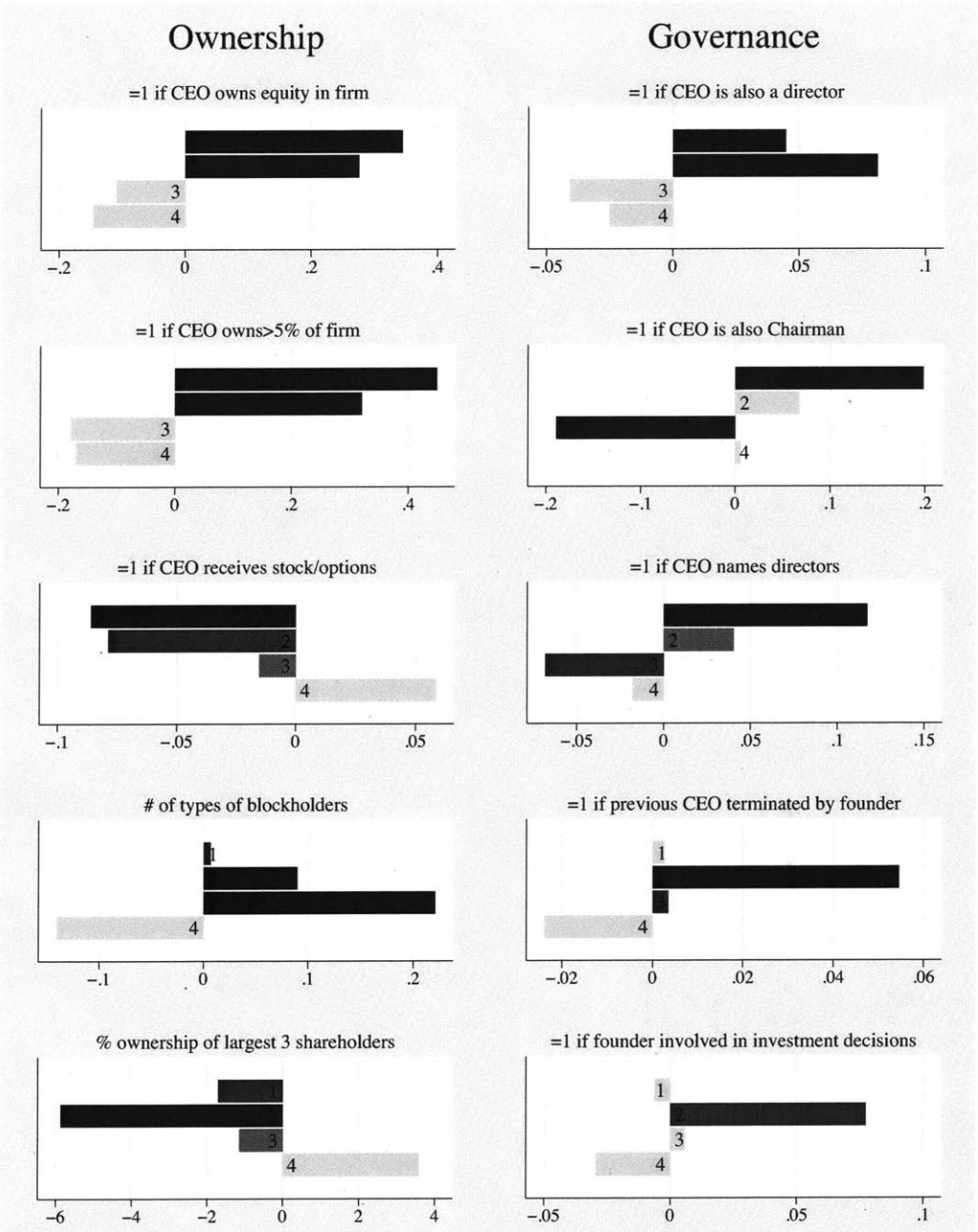


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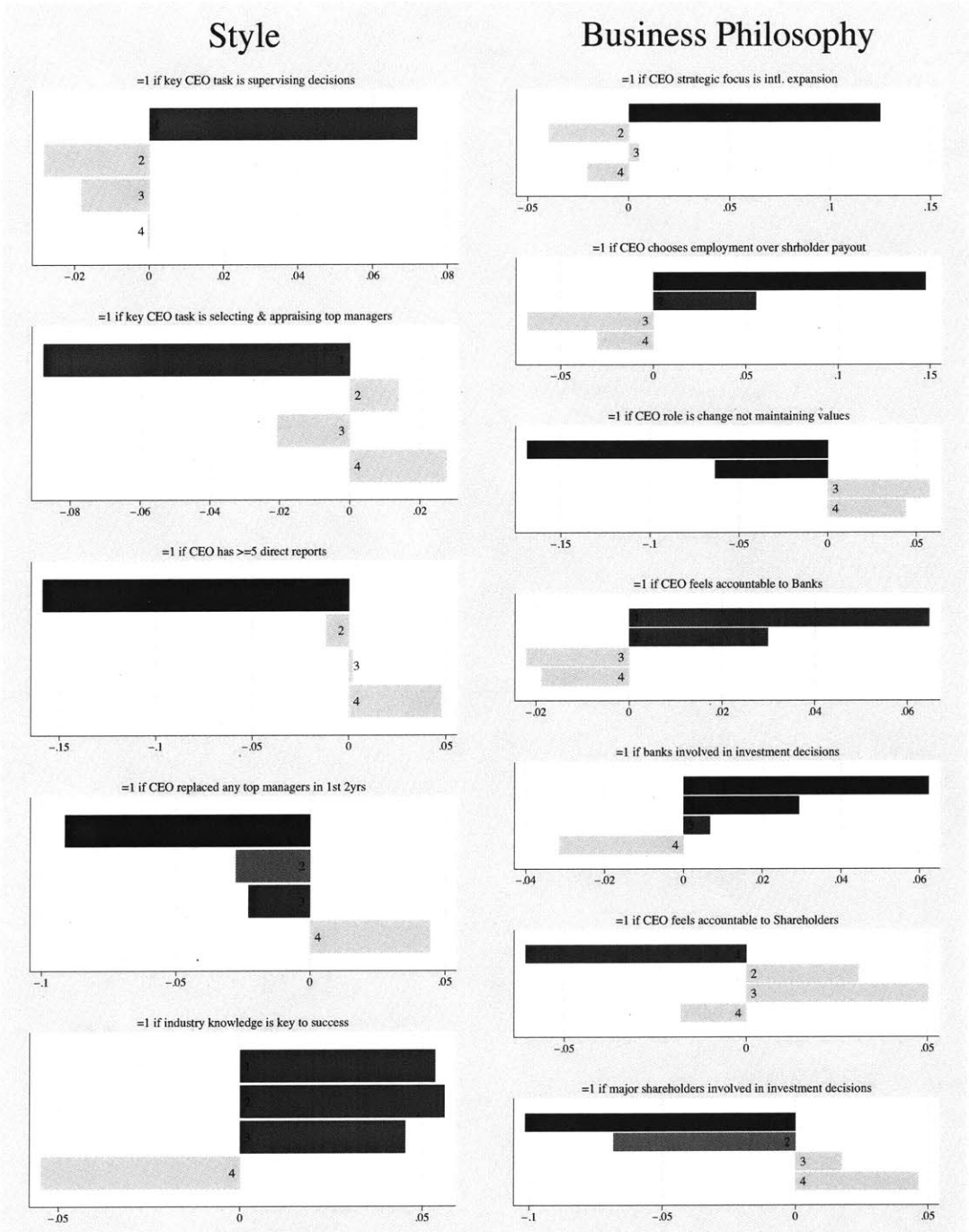
Figure 3



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Online Appendix

Table 1: # of Survey responses by Country and Region

Country	# observations	%
Argentina	49	6%
Brazil	52	6%
Chile	38	5%
Colombia	50	6%
Costa Rica	21	3%
Ecuador	45	5%
Egypt	32	4%
El Salvador	31	4%
Ghana	26	3%
Guatemala	30	4%
Hong Kong, China	15	2%
India	30	4%
Kenya	60	7%
Malaysia	20	2%
Mexico	46	6%
Nigeria	29	4%
Peru	75	9%
Singapore	18	2%
South Africa	26	3%
Turkey	46	6%
Venezuela	47	6%
Zimbabwe	37	4%
TOTAL	**823**	**100%**

Region	# countries	%
S. America	7	32%
C. America	4	18%
Africa	5	23%
East Asia	4	18%
Turkey-Egypt	2	9%
TOTAL	**22**	**100%**

Table 2: Ordered Probit Regressions

	Number of blockholder types				Number of managers reporting directly to CEO		
Response:	0	1	2	3+	< 5	5 to 10	10+
Founder CEO	-0.07***	-0.03**	0.07***	0.03**	0.15***	-0.05**	-0.10***
	[-3.29]	[-2.10]	[3.45]	[2.49]	[4.08]	[-2.12]	[-5.48]
Related CEO	-0.08***	-0.07***	0.10***	0.05***	0.04	-0.01	-0.04
	[-4.76]	[-3.26]	[5.51]	[3.66]	[1.14]	[-0.79]	[-1.18]
Prof. CEO of Family firm	-0.10***	-0.13***	0.14***	0.09***	0.03	-0.00	-0.03
	[-5.40]	[-5.19]	[7.71]	[4.39]	[1.43]	[-0.89]	[-1.43]
Country-level controls	Y	Y	Y	Y	Y	Y	Y
Sales & public listing controls	Y	Y	Y	Y	Y	Y	Y
SIC code fixed effects	Y	Y	Y	Y	Y	Y	Y
Observations	750	750	750	750	802	802	802

The table reports estimated marginal effects (by CEO type) from Ordered Probit regessions of two (ordered) survey responses for each potential response. The independent variables are: three CEO type indicator variables (the omitted category is professional CEO of a non-family firm), four country-level variables, two-digit SIC code fixed effects, an indicator for whether the firm is publicly listed, the natural logarithm of firm sales in 2006, and an indicator for missing value of firm sales. The country level variables are: the natural logarithm of per capita GDP, the Transparency International corruption index (higher values indicate low corruption), the Heritage Foundation property rights index (higher scores indicate more secure property rights), and an indicator variable for English or French legal origin (1 denotes English). t-statistics are in parentheses below each coefficient. *, **, and *** denote coefficients significant at 10%, 5%, and 1% respectively. Standard errors are clustered by country.

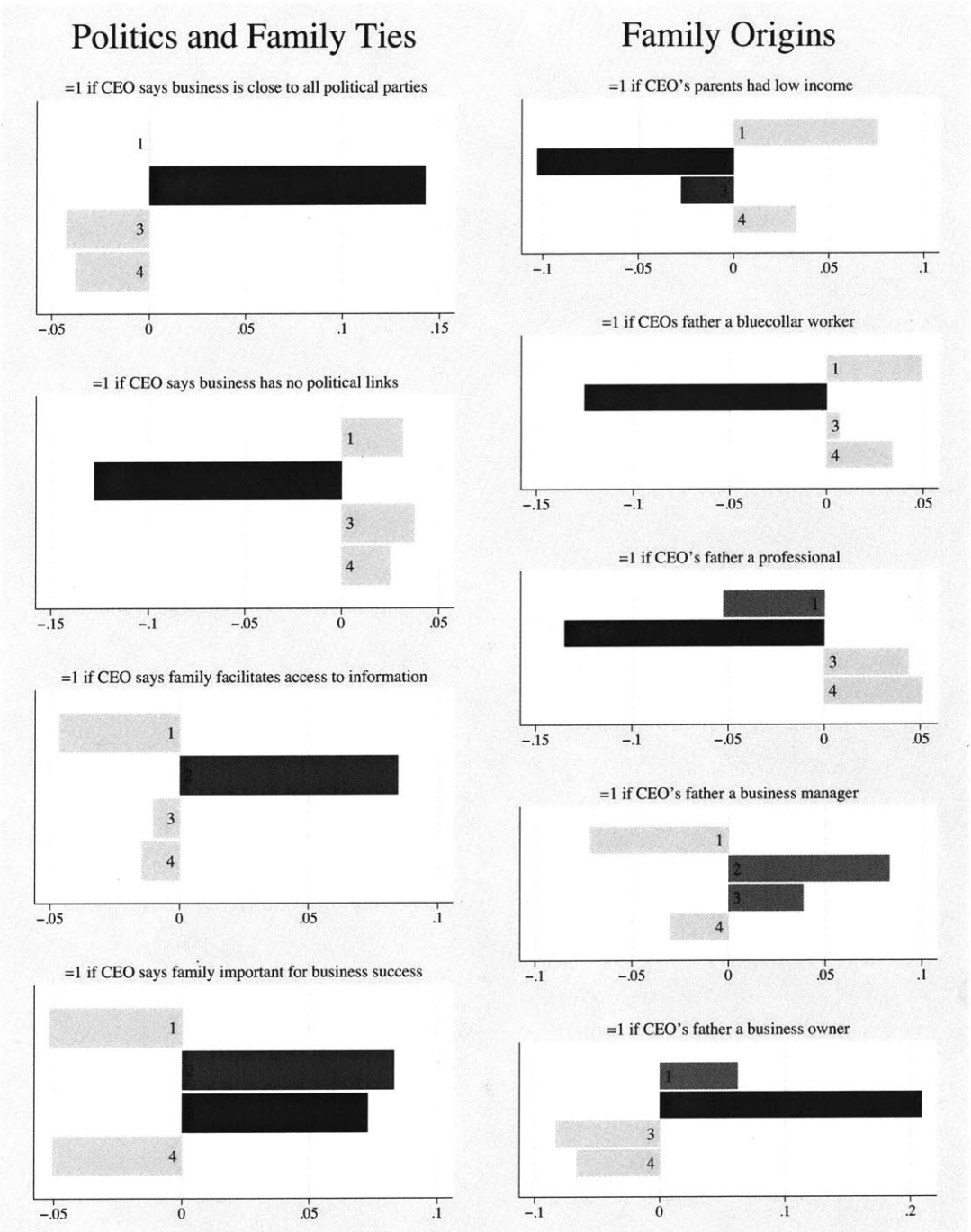
Table 3: Politics & Family Ties, and Family Origins

Panel A **Politics and Family Ties**	Business tries to maintain relationship with all political parties	Business generally does not have close relationships with any political party or candidate	Family relationships facilitate access to information	Family relationships are very/moderately important in business
Founder CEO	0.06	-0.01	-0.03	0.01
	[0.890]	[-0.132]	[-0.403]	[0.125]
Related CEO	0.218***	-0.182***	0.115**	0.161**
	[4.318]	[-3.486]	[2.749]	[2.673]
Prof. CEO of Family firm	0.00	0.00	0.01	0.143***
	[0.066]	[0.080]	[0.160]	[3.670]
Country-level controls	Y	Y	Y	Y
Sales & public listing controls	Y	Y	Y	Y
SIC code fixed effects	Y	Y	Y	Y
Observations	720	720	821	796
Adjusted R2	0.06	0.06	0.03	0.03

Panel B **Family Origins**	Parental income when growing up was low	Father was a blue collar worker	Father was a professional	Father was a business manager	Father was a business owner
Founder CEO	0.04	0.01	-0.14*	-0.04	0.17*
	[0.978]	[0.195]	[-1.876]	[-1.304]	[1.894]
Related CEO	-0.16***	-0.19***	-0.23***	0.13*	0.34***
	[-5.700]	[-5.333]	[-5.237]	[1.812]	[4.907]
Prof. CEO of Family firm	-0.07**	-0.04	-0.02	0.08*	-0.01
	[-2.674]	[-1.091]	[-0.415]	[2.052]	[-0.195]
Country-level controls	Y	Y	Y	Y	Y
Sales & public listing controls	Y	Y	Y	Y	Y
SIC code fixed effects	Y	Y	Y	Y	Y
Observations	759	693	693	694	694
Adjusted R2	0.11	0.06	0.04	0.01	0.09

Regressions of survey responses on a constant, three CEO type indicator variables (the omitted category is professional CEO of a non-family firm), four country-level variables, two-digit SIC code fixed effects, an indicator for whether the firm is publicly listed, the natural logarithm of firm sales in 2006, and an indicator for missing value of firm sales. The country level variables are: the natural logarithm of per capita GDP, the Transparency International corruption index (higher values indicate low corruption), the Heritage Foundation property rights index (higher scores indicate more secure property rights), and an indicator variable for English or French legal origin (1 denotes English). All dependent variables are indicator variables, taking a value of 1 if the respondent is in agreement with the question or answers in the affirmative, and a 0 otherwise. Regressions are estimated using the linear probability model. t-statistics are in parentheses below each coefficient. *, **, and *** denote coefficients significant at 10%, 5%, and 1% respectively. Standard errors are clustered by country.

Figure 1



Bars are means of regression residuals after controls (as described in corresponding tables) by the following CEO types: 1: CEO Founder; 2: Related CEO; 3: Professional CEO of a Family Firm; 4: Professional CEO of a Non–Family firm. Bars of darker colors denote significant differences from the baseline category (4) in the corresponding regression tables.

Paternal-Grandfather-to-Father Occupational Transition Matrices

We examine the occupational transitions between the fathers and paternal grandfathers of the CEOs in our sample. These transition matrices are unusual in that we are looking back from a highly selected sample of CEOs to the occupational transitions of their fathers and paternal grandfathers. The aim is to understand how much income and occupational mobility happened in generations prior to the current period, or whether the current CEO represents a unique jump in attainment that could have occurred from any point in the occupation distribution.

52% of CEOs' fathers worked in a different occupational category than *their* fathers (i.e., CEOs' paternal grandfathers). While this may appear to be a high degree of mobility, much is driven by the secular shift away from farming into other occupations that occurred across the world. **Table** 4a shows CEOs' fathers' occupations as a percentage of their fathers' occupations and should be read by rows rather than by columns: i.e., for all professional paternal grandfathers, 47% of their sons (whose sons, in turn, were CEOs) were also professionals, while 6% became blue-collar workers and 35% went into business.

Whatever the occupation of CEOs' paternal grandfathers, there was a striking movement by the **fathers** of CEOs into the business occupation category: it is the largest occupational category choice for all except those fathers whose own father was a professional, and even in that case 35% chose to work in business.[1] Thus only 22% of blue-collar grandfathers had a blue-collar son in our sample, and the proportions are similar for grandfathers that were farmers and Government officials.[2] In conjunction with the very low proportion of CEOs with a blue-collar father (15%), or low parental income (14%), this suggests it was difficult to move from the lower strata of society to CEO positions in a single generation.

One way to think about this matrix is to consider what would occur if this transition matrix for CEOs' fathers and grandfathers (in Table 4a) was a steady state transition matrix for a population. In such a scenario, what proportion of each occupational class would result from it in the long run? By iterating it hundreds of times, we obtain the steady state distribution of people across the five occupation classes, displayed in **Table** 4b.

[1] For all transition tables, the pattern and magnitudes are essentially unchanged if we exclude related CEOs because of concerns that the transition matrix of related CEOs is less informative (as we already know that many of their fathers and/or grandfathers were CEOs).

[2] The status of farmers and Government workers in CEOs' grandparents' generation is unclear, so moving from those occupations and into business may not necessarily have implied an improvement in social standing.

This means that, if the people in our CEO sample were just like their fathers, and faced similar obstacles and opportunities, we would have seen a distribution somewhere in between the two in the table above. That is, we would have observed between 55% and 65% of our sample in business occupations, instead of observing 100% in business. As this is a sample of people who are selected based on their CEO position – which suggests natural ability coupled with opportunity – this difference between the steady state proportion in business suggested by their fathers' occupational transition matrix and the actual proportion does not seem unduly large. Indeed, perhaps the opposite: it may be evidence in favor of the inter-generational transmission of CEO ability, i.e., that their fathers were, as a group, already showing evidence of above-average talent for business.

When we draw up the paternal grandfather's occupation to father's income transition matrix in **Table5**, we see a clear grouping of fathers in the middle income category, regardless of the grandfather's occupation. The particularly low proportion of low income fathers with the grandfather in business (7%) suggests that the latter were already at least moderately successful, if some economic advantage is transmitted between generations as the literature suggests. At the other extreme, approximately half of low income fathers had farmer fathers themselves (i.e., CEO paternal grandfathers).

The literature on family firms has also focused on the distance in generations between the founder and the current controlling family members. For example, in their study of Thai family business groups Bertrand et al. (2008) note that they have been around for an average of 2.5 generations, while Villalonga and Amit (2006) report of their sample of Fortune 500 firms that approximately a third were in their first generation, a further third were in their second generation, and the remainder were older still. While this information is not available in our sample for family firms run by professional CEOs, it is available for firms with related CEOs: approximately 60% of related CEOs are a single generation younger than the founder (i.e., are the son, daughter, or nephew), while a further 18% are two generations younger (i.e., grand-children of the founder).[3]

[3] The remaining related CEOs are either in the same generation as the founder – 4% of related CEOs – (e.g., wife, brother, sister) or their generation could not be determined from their survey responses.

Table 4

(a) Inter-generational occupational transition matrix for CEO fathers and grandfathers

Origin: CEOs' paternal grandfathers' occupation	Destination: CEOs' fathers' occupation					
	Blue-collar	Professional	Business	Govt.	Farmer	*Total*
Blue-collar	22%	7%	**54%**	16%	1%	100%
Professional	6%	**47%**	**35%**	10%	2%	100%
Business	4%	11%	**78%**	5%	1%	100%
Govt.	8%	22%	**43%**	24%	4%	100%
Farmer	16%	18%	**35%**	8%	24%	100%

The table presents the inter-generational transition matrix of occupations for CEOs' father and grandfathers. Rows sum to 100%, so for each category of paternal grandfather occupation, the table presents the distribution of their sons' (the CEOs' fathers') occupations. The diagonal, representing relative occupational stability over generations, is outlined. Cell values over 30% are in bold.

(b) Steady State vs Actual distribution of CEOs' fathers

Distribution:	Actual (CEOs' fathers')	Steady state
Blue-collar	10.5%	6.2%
Professional	17.8%	18.3%
Business	54.3%	65.1%
Govt.	9.3%	8.3%
Farmer	8.1%	2.1%

The first column of this table shows the actual distribution of CEOs' fathers' occupations. The second column iterates the occupational transition matrix in the table above to obtain the steady state distribution of occupations suggested by the transition matrix.

Table 5: Inter-generational pseudo-transition matrix

Origin: CEOs' paternal grandfathers' occupation	**Destination: CEOs' fathers' occupation**			
	Low	Middle	High	Total
Blue-collar	20%	**63%**	17%	100%
Professional	11%	**55%**	**34%**	100%
Business	7%	**44%**	**48%**	100%
Govt.	12%	**51%**	**37%**	100%
Farmer	24%	**54%**	23%	100%

The table presents the inter-generational pseudo-transition matrix from CEOs' paternal grandfathers' occupations to their sons' (the CEOs' fathers') income levels when the CEO was growing up. Rows sum to 100%, so for each category of paternal grandfather occupation, the table presents the distribution of their son's income. Cell values over 30% are in bold.

Survey on Business Perspectives of Top Executives

COMPANY INFORMATION

Company name: «CompanyName»

Year of incorporation: _____

What is this company's main sector of operation? Please select one:

- ☐ Agriculture
- ☐ Automobile
- ☐ Construction
- ☐ Consumption Goods
- ☐ Corporate Services
- ☐ Energy
- ☐ Finance
- ☐ Pharmaceuticals
- ☐ Food processing
- ☐ Household Equipment
- ☐ Non-Traded Services
- ☐ Real Estate
- ☐ Retail and Wholesale Trade
- ☐ Transportation
- ☐ Mining/Metals
- ☐ Other _____

Company sales in 2002 (in millions of U.S. dollars): $_____

PERSONAL INFORMATION

Name: «Firstname» «Lastname»

Full job title:_____

Year of birth: 19____

Place of birth: city:_____
country:_____
Nationality:_____

Gender: ☐ Male ☐ Female

EDUCATIONAL BACKGROUND

Do you have an undergraduate university degree? ☐ Yes ☐ No
If yes, please specify:
School attended:_____
Location: city:_____
country:_____

Field of study: _____

Do you have a Masters Degree in Business Administration (MBA)? ☐ Yes ☐ No
If yes, please specify:
School attended: _____
Location: city: _____
country: _____

Do you have any other postgraduate university degree? ☐ Yes ☐ No

If yes, please specify:
Degree type: ☐ Ph.D. ☐ Masters
Field of study: _____
School attended: _____
Location: city: _____
country: _____

Did you ever attend a military school? ☐ Yes ☐ No

PRIOR WORK EXPERIENCE

In which year did you first join «CompanyName»? _____

In which position did you first join «CompanyName»? _____

In which year did you become the «title» of «CompanyName»? _____

Please list the three positions (business and non-business related, academia, government, military etc.) you held the longest prior to becoming «title» of «CompanyName»:

Job Title	Company Organization	Start Year	End Year	Country
1.				
2.				
3.				

Who appointed you as the «title» of «CompanyName»? Please select one:

☐ Board of directors ☐ Previous CEO

☐ Large Shareholder(s) ☐ Government officials
☐ Founder or relatives of founder ☐ CEO of a parent company
☐ Other (please specify):_____ ☐ Nominating Committee

Do you sit on your company's board of directors? ☐ Yes ☐ No

Are you the chairman of the board? ☐ Yes ☐ No

YOUR BUSINESS APPROACH

Which of the following alternatives best describes your strategic focus for «CompanyName» in the next five years? Please select one.

☐ Diversify into new industries
☐ Expand into international markets
☐ Strengthen focus in core businesses

Which of the following two alternatives best describes your view? Executives should:

☐ Maintain payments to shareholders, even if they must lay off a number of employees
☐ Maintain stable employment, even if they must reduce payments to shareholders

As the «title» of «CompanyName», who do you feel the most accountable to? Please select *up to two* alternatives:

☐ Customers ☐ Parent Company
☐ Shareholders ☐ Employees
☐ Government ☐ Banks and other major lenders
☐ Other (please specify): _____

As the «title» of «CompanyName», what do you perceive as your most important *operational* tasks? Please select *up to two* alternatives:

☐ Selecting and appraising other top managers in the company
☐ Supervising operational, strategic, and financial planning decisions
☐ Managing the company image and reputation with outsiders (such as customers, media,
banks, and other investors)

☐ Representing the interests of the company in front of government and regulatory bodies
☐ Other (please specify):_____

How would you characterize your leadership role in «CompanyName»? Your task is to:

☐ Bring about changes in the way the company is run
☐ Guarantee the stability of the company's traditions and values

Consider the following tasks. At «CompanyName», how would you characterize the way top level decisions are made for each of these tasks? For each category, please select one.

1. Selection and appraisal of top employees are:

☐ Made by myself
☐ Delegated to other top executives with my final say
☐ Made jointly by a group of top executives including myself
☐ Made jointly by members of the board and myself
☐ Other (please specify): _____

2. Operational, strategic and business planning decisions are:

☐ Made by myself
☐ Delegated to other top executives with my final say
☐ Made jointly by a group of top executives including myself
☐ Made jointly by members of the board and myself
☐ Other (please specify): _____

3. Financial planning and budgeting decisions are:

☐ Made by myself
☐ Delegated to other top executives with my final say
☐ Made jointly by a group of top executives including myself
☐ Made jointly by members of the board and myself
☐ Other (please specify): _____

Which of the company's stakeholders are you most likely to involve before deciding to undertake a large-scale investment project, such as the acquisition of a plant or a company? Please select up to two.

- ☐ Bankers
- ☐ Parent company CEO
- ☐ Government
- ☐ Other top executives in the company
- ☐ Other (please specify): _____
- ☐ Larger shareholders
- ☐ Founder
- ☐ Members of the board
- ☐ I do not involve anyone

How many managers in your company report *directly* to you? Please select one.

- ☐ Less than five
- ☐ Five to ten
- ☐ Ten to fifteen
- ☐ Fifteen to twenty
- ☐ More than twenty

How many of the upper-level managers did you replace in the first two years after you took office as the «title» of «CompanyName»? Please select one.

- ☐ All
- ☐ More than half
- ☐ Less than half
- ☐ None

How many hours do you work in a typical week? Please select one.

- ☐ Forty hours or less
- ☐ Forty to fifty hours
- ☐ Fifty to sixty hours
- ☐ Sixty to seventy hours
- ☐ More than seventy hours

FAMILY BACKGROUND

For how many generations has your family been involved in the upper-level management of large businesses? Please select one.

- ☐ One generation
- ☐ Two generations
  generations
- ☐ Three generations
- ☐ More than three

How would you classify the income level of your parents when you were growing up (compared to other families in your country of residence)? Please select one.

- ☐ Lower income
- ☐ Middle income
- ☐ Upper income

What is/was your father's main occupation?

- ☐ Large business manager (>100 employees)
- ☐ Small business manager (<100 employees)
- ☐ Large business owner (> 100 employees)
- ☐ Small business owner (<100 employees)
- ☐ Administrative Clerk
- ☐ Sales person
- ☐ Other (please specify): _____
- ☐ Medical Doctor
- ☐ Engineer/Scientist
- ☐ Teacher/Professor
- ☐ Government Official
- ☐ Judge/Lawyer
- ☐ Farmer

What is/was your paternal grandfather's main occupation?

- ☐ Large business manager (>100 employees)
- ☐ Small business manager (<100 employees)
- ☐ Large business owner (> 100 employees)
- ☐ Small business owner (<100 employees)
- ☐ Administrative Clerk
- ☐ Sales person
- ☐ Other (please specify): _____
- ☐ Medical Doctor
- ☐ Engineer/Scientist
- ☐ Teacher/Professor
- ☐ Government Official
- ☐ Judge/Lawyer
- ☐ Farmer

What is/was your maternal grandfather's main occupation?

- ☐ Large business manager (>100 employees)
- ☐ Small business manager (<100 employees)
- ☐ Large business owner (> 100 employees)
- ☐ Small business owner (<100 employees)
- ☐ Administrative Clerk
- ☐ Sales person
- ☐ Other (please specify): _____
- ☐ Medical Doctor
- ☐ Engineer/Scientist
- ☐ Teacher/Professor
- ☐ Government Official
- ☐ Judge/Lawyer
- ☐ Farmer

In many countries around the world, mutual support of family members in business transactions is essential for efficient business operations. In your view, how important are family relationships for conducting successful business in your country? Please select one.

- ☐ Very important
- ☐ Moderately important
- ☐ Not important at all
- ☐ A hindrance to success

In your view, which of following transactions are most commonly facilitated through family relationships in your country? Please select *up to two* options:

- ☐ Access to business information
- ☐ Access to credit for company
- ☐ Dealing with government and regulators
- ☐ Hiring of top managers and other employees
- ☐ Sale and purchase of assets
- ☐ Supplier and customer relationships
- ☐ Other (please specify):_____

Are you the founder of «CompanyName»? ☐ Yes ☐ No

Are you a relative of «CompanyName»'s founder? ☐ Yes ☐ No
If yes, please specify family relationship:

- ☐ Son/Daughter
- ☐ Grandson/Granddaughter
- ☐ Brother/Sister
- ☐ Other (please specify):

_____

Were any of your relatives ever employed in an upper-level management position at «CompanyName»? Please specify:

- ☐ Father
- ☐ Father-in-law
- ☐ Uncle
- ☐ Other (please specify):_____
- ☐ Maternal grandfather
- ☐ Paternal grandfather
- ☐ Brother/Sister

Are/were any of your following relatives a majority equity owner (at least 20 percent ownership stake) in «CompanyName»? Please specify:

- ☐ Father
- ☐ Father-in-law
- ☐ Uncle
- ☐ Other (please specify):_____
- ☐ Maternal grandfather
- ☐ Paternal grandfather
- ☐ Brother/Sister

COUNTRY CULTURE

Suppose that the government is deliberating to pass a new law that would substantially hinder business activities in your industry. What mechanisms, if any, do business leaders in your country have to prevent the government from making a major mistake? Please choose the answer that most closely reflects your view:

☐ There is no way to influence the government's decisions
☐ There is a formal lobbying process through which firms can exchange information and
opinions with the government
☐ The government will generally consult business leaders in advance about any changes that
may affect their industry
☐ Most business leaders have contacts in the government who listen to their opinions

In general, how would you describe the current government's attitude towards business in your country? Please specify one.

☐ Supportive ☐ Indifferent ☐ Hostile

How would you describe the relationship between major companies and political parties in your country? Most business leaders:

☐ Try to maintain close relationships with all political parties and candidates
☐ Support only one political party or candidate and do not maintain close relationships with
other parties
☐ Do not have close relationships with any political party or candidate

Which do you consider to be the most important factors to being a successful «title» in your country? Please select the *up to two* alternatives:

☐ Business contacts
☐ Family background and family contacts
☐ Formal business training and business experience
☐ Political contacts
☐ Specific industry knowledge
☐ Ability to communicate ideas and persuade others
☐ Other (please specify):_____

In your view, which of the following occupations carries the most social prestige in your country? Please select the *top two* alternatives:

- ☐ Business manager
- ☐ Engineer
- ☐ Entrepreneur
- ☐ Other (please specify):_____
- ☐ Lawyer
- ☐ Medical doctor
- ☐ Government official

COMPANY STRUCTURE

As the «title» of «CompanyName» do you hold equity in the firm (stock options)?

- ☐ Yes, I hold more than 5% of the company's stock
- ☐ Yes, I hold less than 5% of the company's stock
- ☐ Yes, I receive stock and stock options as part of my compensation
- ☐ No, I do not hold equity in the firm

Is your company majority-owned by a parent corporation? ☐ Yes ☐ No

If yes, please give the name of that parent corporation:_____

How concentrated is the ownership of your company? That is, what fraction of equity in your company is held by the three largest shareholders?

Shareholder 1 _____%
Shareholder 2 _____%
Shareholder 3 _____%

Please indicate if any of the three largest equity holders is/are:

- ☐ The founder or relatives of the founder
- ☐ Foreign investors
- ☐ Foreign corporations
- ☐ Domestic corporations
- ☐ The government

In general, how are other top managers appointed in this company? Mostly through:

- ☐ Internal promotions
- ☐ External hires
- ☐ Both

Who appoints the board members in your company? Please choose *up to three* alternatives.

- ☐ I select most of the board members
- ☐ Company founder
- ☐ CEO of the parent company
- ☐ Large shareholders
- ☐ Government
- ☐ Other, please specify: _____

Why did the previous «title» leave «CompanyName»?

- ☐ Retired for age, health or family-related reasons
- ☐ Decided to leave for a non-business position
- ☐ Decided to leave for another business position
- ☐ Board of directors terminated his appointment
- ☐ Company founder terminated his appointment
- ☐ CEO of a parent company terminated his appointment
- ☐ The government terminated his appointment
- ☐ Other (please specify):_____

Chapter 3
How Corporate Political Contributions Pay Off*

> "In 2002, Westar [Energy] coordinated a series of contributions by the company and its top executives to influential members of Congress and their allies. These donations—in a memo, the VP for public affairs specified the dollar amounts to be given by at least a dozen executives—were timed to help ensure that legislators would include a provision beneficial to Westar in the annual comprehensive energy bill, then in the late stages of congressional consideration. The executives made the recommended contributions, and one of the targeted congressmen inserted Westar's requested exemption into the bill."
>
> —*The Conference Board Review* (January 2012, p. 25)

1 Introduction

Firms contribute to U.S. political election campaigns, but why they contribute remains unclear. A natural explanation—consistent with the quote above—is that they donate as part of a strategy to obtain favorable legislative outcomes or an increased share of federal outlays. Alternatively, corporate campaign contributions may reflect the political preferences of managers, representing a form of perquisite consumption that may in fact harm rather than benefit the company's shareholders (Jensen and Meckling, 1976; Aggarwal et al., 2012). The Supreme Court's 2010 *Citizens United* ruling, which substantially relaxed constraints on political spending by corporations, has only increased the importance of understanding what drives corporate political contributions (Coates, 2012; Bebchuk and Jackson, 2013).

Endogeneity concerns make it difficult to bring evidence to bear on this question: because political contributions are freely chosen by firms, they may be driven by unobserved firm

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characteristics (such as expected profitability) that also drive future firm performance, making it difficult to establish causality. For instance, Cooper et al. (2010) document a positive correlation between campaign contributions and firm value, suggesting that political connections are beneficial to companies. However, as the authors acknowledge, such evidence does not necessarily warrant a causal interpretation.

This paper addresses this concern by focusing exclusively on very close U.S. congressional elections using a regression discontinuity design (RDD).[1] I define close elections as those decided by a margin of less than 1% of votes between the winner and loser. Such close-call elections provide a quasi-natural experiment in which the elected representatives (and hence the contributing companies) are "as good as randomly assigned" (Lee and Lemieux, 2010). Accordingly, these close elections provide plausibly exogenous variation in companies' political connections.

In addition to using only close elections, I further focus on corporate contributions to "local" candidates, i.e. candidates running in the state where the supporting companies conduct most of their operations. The importance of connections to local politicians has been emphasized in several recent articles (e.g., Faccio and Parsley, 2009; Cooper et al., 2010; Cohen et al., 2012; Ovtchinnikov and Pantaleoni, 2012; Asher and Novosad, 2013). Anecdotal evidence also indicates that politicians are most likely to stand up for companies in their local constituency. For example, Michigan Representative Guy Vander Jagt noted in an interview that "I have one Fortune 500 company in my district [...] I always knocked myself out for them because they were the biggest employer in that county. Their health was essential to the health of my constituents, the people who worked there" (Schram, 1995, cited in Ḃombardini and Trebbi, 2011, p. 588).

To measure political connections, I obtain information on campaign donations made by companies' political action committees (PAC) to their local candidates.[2] I then match candidates to congressional elections from 1980 to 2010 in which the winning margin is less than 1% of votes. There are 101 such close elections corresponding to 604 donor firms. Firms supporting winners and losers are almost uniformly distributed (299 firms supporting winners, 305 supporting losers) and are very similar on the basis of observable

[1] The RDD is widely used in the economics and political science literature (for a survey, see Lee and Lemieux, 2010) and has become increasingly popular in financial economics as well (see Roberts and Whited, 2013, Chapter 5). As Lee and Lemieux (2010) argue, the RDD has more in common with randomized experiments than IV, matching, or difference-in-difference approaches, and as such is the non-experimental method best suited to causal inference.

[2] PAC contributions are commonly used in the literature to measure political connections in the U.S. corporate sector (see, e.g., Goldman et al., 2009; Cooper et al., 2010; Aggarwal et al., 2012). For details on PACs, see Section 2.1.

characteristics prior to the election. This evidence strongly supports the validity of the RDD—companies are effectively randomly assigned to winning and losing politicians.

My RDD estimates indicate that contributing to a winning candidate increases firm value (as measured by Tobin's Q) by 2.3% to 4.8% relative to contributing to a losing candidate. This finding is robust to a wide range of alternative specifications of the RDD, such as the use of polynomials of the running variable, different kernels for the local linear regression, including a variety of covariates and fixed effects (year, industry, election), changing the sample bandwidth to up to three times that of the main specification, reducing it to half, using Imbens and Kalyanaraman's (2012) optimal bandwidth, minimizing mean absolute deviations instead of the sum of squared deviations, and excluding companies that contribute to multiple candidates. Finally, my results also hold if I examine the short-term stock market reaction around the announcement of the election outcome instead of the long-term changes in firm value. Specifically, I find that the difference in the three-day cumulative abnormal return of companies supporting winners compared to companies supporting losers is 1.1%. Overall, this evidence is consistent with the view that companies benefit from political connections.

The RDD framework ensures that companies are locally randomized around the winning threshold, and thus whether a company supports an ex post winning or losing candidate is likely to be randomized. That being said, *which* candidate a politically active firm chooses to support is not randomized. Accordingly, a potential concern is that what is driving my results is not that winning candidates "repay"companies for their support (in a quid pro quo relationship), but rather that companies choose to support candidates that are ideologically well-disposed to their activities, and thus any ex post benefits the company obtains would have occurred even in the absence of the link between the company and the winning politician. One way to distinguish between these two interpretations is to examine firms that do not make political campaign contributions and yet are in the same state and industry as firms contributing to winning and losing candidates. If what is driving my results is alignment between candidates and specific industries, I should find a similar effect at these companies as well. Nevertheless, on examining these companies I find that the effect on firm value, albeit positive, is small and insignificant. This finding supports the interpretation that being connected to a winning candidate has a direct payoff to the donor companies.

Next I study how the results vary across industries. I find that the increase in firm value is highest for firms operating in industries with a high share of government procurement contracts (e.g., defense) and in industries in which regulations are particularly compre-

hensive (e.g., banking, telecommunications). This finding is not surprising given that, in these industries, business strategy and revenues are directly linked to political decisions (see Coates, 2012, for a similar argument).

The effect also appears stronger for firms whose campaign contribution is greater than the median amount. In contrast, I find no change in firm value if instead of considering contributions to local candidates I consider donations to out-of-state candidates (e.g., a company whose operations are located in California contributing to a New Hampshire candidate). The latter finding underscores the importance of geography in understanding the payoff from political contributions.

I then examine four channels through which political connections may increase firm value: 1) allocation of procurement contracts, 2) mitigation of legislative and regulatory risks, 3) improved access to bank financing, and 4) tax advantages. Using the RDD framework, I document evidence consistent with the first three channels. This evidence is again strongest in government-dependent industries, i.e. in industries with a high government share and in industries in which regulations are particularly comprehensive. In contrast, I find no evidence suggesting that companies take advantage of their political connections to achieve a more favorable tax treatment.[3]

Given the payoffs to contributing to the winning candidate, it is surprising that the average corporate contribution is approximately $2,000 in nominal dollars in the sample ($3,000 in 2010 dollars). This is part of a long-standing but unresolved question in political science dating back to Gordon Tullock, who posed the question of why there is so little money in U.S. politics given the size of the potential payoffs (Tullock, 1972). Relatively strict campaign finance limits were in place during the sample period (see Section 2.1 for details), which could explain why average contribution amounts are low. Nevertheless, Ansolabehere et al. (2003) argue that firms had substantial scope for subsidizing their affiliated PACs which they systematically did not exploit, suggesting that campaign finance constraints were not entirely binding.

[3] My evidence in support of the first three channels is related to the findings of other studies, although none of these studies uses a RDD based on close elections. Specifically, the procurement channel is consistent with Goldman et al. (2012) who document that the board membership of a former politician increases the amount of government procurement contracts allocated to the firm after the shift in political control of the U.S. Congress in 1994. Evidence related to the second channel is provided in Yu and Yu (2011), who find that firms' lobbying activities are associated with a lower likelihood of fraud detection. Similarly, Fulmer and Knill (2012) document that accused executives at firms that make political contributions appear to face less severe penalties from the Securities and Exchange Commission (SEC) and the Department of Justice (in the case of criminal penalties). As for the third channel, Claessens et al. (2008) document that firms contributing to winning candidates in Brazilian federal elections are able to increase their bank financing following the elections. Similar evidence is provided by Khwaja and Mian (2005) and Houston et al. (2012) for Pakistan and the U.S., respectively.

A potential explanation for this apparent puzzle is that campaign contributions are part of a more complex relationship between firms and elected politicians—the "tip of the iceberg"so to speak. In addition to providing campaign contributions, companies provide other benefits to elected officials.[4] In particular, companies engage in lobbying activities (see, e.g., Bertrand et al., 2012). In line with the iceberg view of campaign contributions, there is a strong association between campaign contributions and lobbying expenditures, and the latter are an order of magnitude greater than campaign contributions (Ansolabehere et al., 2002).[5] Accordingly, it could be that campaign contributions open doors to a more active political engagement of the donor firms—companies whose "political ally" is elected may have stronger incentives to engage in lobbying, even if doing so is costly. This hypothesis has been suggested in the political science literature where it is known as the "access view" of campaign contributions (e.g., Ansolabehere et al., 2002). In this view, campaign contributions provide access to legislators and hence an entry point for subsequent lobbying efforts.

To examine whether companies increase their lobbying activities following a close victory of their candidate, I manually collect lobbying data and study changes in lobbying expenses before and after the close elections. Companies contributing to winners raise their lobbying expenditures relative to firms contributing to losing candidates. This evidence is in line with the iceberg view of campaign contributions—seemingly small campaign contributions reflect the tip of the iceberg of a more substantial political engagement of companies.

This paper is related to the growing literature that examines the relationship between companies and politicians by studying corporate campaign contributions (e.g., Stratmann, 1998, 2002; Cooper et al., 2010; Aggrawal et al., 2012; Coates, 2012), personal ties between politicians and senior executives (e.g., Fisman, 2001; Johnson and Mitton, 2003), board membership or large equity ownership of former politicians (e.g., Faccio, 2006; Ferguson and Voth, 2008; Goldman et al., 2009, 2012). It is also related to several articles that conduct event studies around the announcement of specific election outcomes or political events (e.g., Roberts, 1990; Jayachandran, 2006; Knight, 2006; Claessens et al., 2008;

[4] Such benefits may include, for example, employment prospects for outgoing elected officials and their staff (Fan et al., 2007; Abramoff, 2011), and favorably timed job creation in election years (Bertrand et al., 2007).

[5] Relatedly, Lessig (2011) argues that the relationship between firms and elected officials functions as a "gift economy," that is, "a series of exchanges between two or more souls who never pretend to equate one exchange to another, but who also don't pretend that reciprocating is unimportant. [...] The key mistake most people make [...] is seeing Washington as a cash economy. It's a gift economy. That's why firms divert money into paying lobbyists rather than spending every dollar on campaign contributions. Campaign contributions are part of the cash economy. Lobbyists are hired because they understand how to participate in the gift economy" (Lessig, 2011, pp. 107-108).

Acemoglu et al., 2010; Fisman et al., 2012). Unlike these articles, my paper relies on a sharp RDD, which provides a quasi-random assignment of donor companies to winning and losing politicians. Such tight identification allows us to study the causal effect of political connections on firm value and other firm-level outcomes. This analysis is made possible by the comprehensive dataset used in this study, which covers all congressional elections from 1980 to 2010 and thus provides a sufficiently large number of close elections.

The remainder of this paper is organized as follows. Section 2 describes the data and empirical methodology. Section 3 presents the main results. Section 4 examines the channels through which political connections enhance firm value. Section 5 examines lobbying expenditures. Finally, Section 6 concludes.

2 Data and Methodology

2.1 Data Sources and Sample Selection

The data on candidate names, districts, and number of votes are obtained from the Clerk of the House of Representatives (CHR). The CHR data cover all U.S. House of Representatives and Senate elections that took place between 1978 and 2010. For each election, I compute the difference in votes between the winner and runner-up (as a percentage of the total number of votes), which I refer to as the "winning margin".[6] The winning margin is the assignment or so-called "running" variable of the RDD, since it "assigns" candidates (and hence companies) to winners and losers. I include all elections in which the winning margin is less than 1% of total votes, although I occasionally use a larger sample where the winning margin is less than 3% of total votes.

To measure companies' political connections, I follow common practice in the literature and focus on donations from corporate Political Action Committees (PAC) to politicians' campaign committees (e.g., Goldman et al., 2009; Cooper et al., 2010; Aggarwal et al., 2012). PACs are organizations that raise and spend funds for political campaigns. A corporate PAC can give a maximum contribution to a candidate campaign committee of $10,000 per election ($5,000 for a primary election and $5,000 for a general election), and

[6] For example, in the 2010 House of Representatives election in Kentucky's 6th congressional district, Democrat candidate Ben Chandler defeated Republican candidate Andy Barr with 119,812 versus 119,164 votes. The total number of votes was 239,223 (the 247 residuals votes went to other candidates). The vote shares of the winner and runner-up were 50.1% and 49.8%, respectively, corresponding to a winning margin of 0.3%.

these funds must be raised exclusively from the firm's executives, shareholders, and their families. Note that corporations themselves cannot contribute to candidate campaign committees directly, nor may they contribute funds to PACs aside from covering the administrative and fundraising costs of affiliated PACs.[7] I obtain data on PACs from the Federal Election Commission (FEC). The FEC data cover all campaign contributions made by corporate PACs to each candidate over the past 16 election cycles from 1979 to 2010. (Since the PAC data start in 1979 and congressional elections take place every two years, my sample covers elections from 1980 to 2010.) I then manually match corporate PACs to corporations in Compustat by company names.

The resulting dataset contains detailed contribution data from the corporate PACs of public firms. For each contribution, the dataset includes the identity of the receiving candidate, the contribution amount and date, and the identity of the contributing firm. I aggregate individual contributions to obtain the total contribution from each firm to each candidate for each close election. I code a company as a "winner" if it supports the winning candidate, and as a "loser" if it supports the runner-up.[8]

I further refine the sample by focusing on donations to "local" politicians, i.e. politicians running in elections in the company's home state. (Contributions to out-of-state politicians are considered separately in Section 3.4.) As discussed above, the focus on local politicians is motivated by the recent literature that emphasizes the importance of connections to local politicians (e.g., Faccio and Parsley, 2009; Cooper et al., 2010; Cohen et al., 2012; Ovtchinnikov and Pantaleoni, 2012; Asher and Novosad, 2013). I code as home state the state in which the company conducts most of its operations. To identify the relevant state for each company, I use the data of Garcia and Norli (2010) on state-level operations of companies based on companies' 10-K filings in the SEC's EDGAR database.[9]

These selection criteria lead to a final sample of 604 firm-election observations corresponding to 101 elections that were decided by a winning margin of less than 1% of total votes; these elections are spread over 32 states and 16 election years (see the details in Appendix

[7] For further details on regulations before *Citizens United*, see Ansolabehere et al. (2003) and Federal Election Commission (2007). For the regime after *Citizens United*, see Federal Election Commission (2011). For information regarding how corporate PAC contributions are distributed among public firms (and their importance to campaign finance), see Cooper et al. (2010).

[8] In about 10% of the firm-election observations in the sample, companies contribute to both the winner and runner-up. In such cases, I code a company as a winner if it contributes more to the winning than to the runner-up candidate (and vice versa). In robustness checks, I show that excluding these observations is immaterial to the results. Lastly, in a few cases (less than 4% of the firm-election observations), the firm contributes the same amount to both candidates. I drop these observations from the sample.

[9] The Garcia and Norli data start in 1994 (the first year in which SEC filings are compiled in EDGAR). For the years prior to 1994, I use the 1994 state.

Table A). These are close elections by any measure: **Table 1** shows that both the mean and median winning margins are approximately 0.5% of votes. The average vote share of the winner and runner-up are 49.2% and 48.7%, respectively. Half of the elections in the sample were won by Democrat candidates, and half by Republicans. The companies donating to winners and losers are described in detail in Section 2.3.

2.2 Methodology

I estimate the causal effect of political connections on firm value by using a regression discontinuity design (RDD). The intuition behind the RDD is as follows. Close elections, i.e. elections that are determined by a very small margin of votes, provide a quasi-random assignment of companies to winners and losers and hence plausibly exogenous variation in political connections. If there is a payoff to political connections, firms that are randomly assigned to the winning candidate will be more likely to realize this payoff than firms assigned to the losing candidate. This difference is what the RDD measures.

The RDD's suitability for causal inference derives from the relatively mild assumptions it requires. The "sharp" RDD used in this paper relies on a single key assumption: companies cannot precisely predict or control the election outcome and thus cannot choose to be connected to the winning politician. This assumption implies that, in the absence of an election ("treatment"), the outcomes of firms just below (bare losers) and above the winning threshold (bare winners) would have been similar, so the only reason that the actual outcomes are different is that after the election some firms are connected to an elected politician ("treated firms"), while others are not ("control firms"). Importantly, this assumption of imprecise control over the election outcome has two testable implications: 1) all observed pre-determined characteristics should be identically distributed on either side of the winning threshold, and 2) the distribution of companies backing winners and losers should be the same. In Section 2.3, I provide both tests and show that they strongly support the assumption of random assignment of winners and losers in the sample of close elections.[10]

[10] One remaining concern is that even if companies are unable to predict or control the election outcome, they may be able to adjust their political connections *after* the election. For example, a company that backed a losing candidate may decide to support the winner after the election, thus offsetting the random assignment to a loser. Nevertheless, this concern is minimized for two reasons. First, to the extent that political connections improve firm value, such behavior would imply that the results understate the true effect of political connections (since my estimate is the difference in outcomes between winner and loser firms). Second, it seems unlikely that politicians would favor companies that support them after the election to the same extent they favor companies that support them from the very beginning. The latter is consistent with recent literature in political science that emphasizes the role of loyalty in politics (e.g., Hasecke and Mycoff, 2007).

To implement the RDD, I pool all firm-election observations and estimate the following regression:

$$\Delta firm\,value = \alpha + \beta \times winner_{ie} + \gamma_1 \times vote_{ie} + \gamma_2 \times (vote_{ie} \times winner_{ie}) + \alpha_t + \varepsilon_{iet}$$

where i indexes firms, e indexes elections, and t indexes election years. Δ firm value is the average Tobin's Q in the two years following the election minus the average Tobin's Q in the two years preceding the election. Tobin's Q is computed from Compustat as the market value of assets divided by the book value of assets, where the market value of assets is the book value of assets plus the market value of common stock minus the sum of the book value of common stock and balance sheet deferred taxes. To ensure that extreme realizations of Tobin's Q are not affecting the results, I winsorize Tobin's Q at the 1st and 99th percentiles of its empirical distribution. Winner is a dummy variable equal to one if the firm contributes to the winning politician and zero otherwise. The coefficient β measures the treatment effect of being assigned to a winner (as opposed to being assigned to a loser) and is therefore the main coefficient of interest.

In regression (1), vote is the running variable of the RDD, i.e. the winning margin centered at zero, such that a positive value indicates that the firm has donated to the winning candidate, while a negative value indicates that the firm has contributed to the losing candidate. Following common practice in the RDD literature, I include vote and vote× winner on the right-hand side of the regression to capture any linear trend in the data on either side of the winning threshold. Accordingly, the RDD is specified as a local linear regression (see Lee and Lemieux, 2010, for details).[11]

The above specification further includes year fixed effects α_t. While fixed effects (of any type) are not required for consistent inference in the RDD, year fixed effects mitigate concerns that certain election years are different from others. In robustness checks, I obtain similar results if I further include election and industry fixed effects to capture unobserved heterogeneity at the election and industry level, respectively. Finally, to account for within-state dependence across observations, I cluster standard errors at the state level. (My results also hold if instead I cluster standard errors at the election level, at the year

[11] Local linear regressions naturally assume a locally linear functional form of the running variable on both sides of the winning threshold, which is a source of bias if the true functional form is not linear. In robustness checks, I show that I obtain similar results if instead of a local linear specification I use a polynomial specification (i.e., by including higher-order terms of vote and vote×winner in equation (1)). In their benchmarking of the RDD against experimental data, Black et al. (2007) report that local linear regressions have lower bias and less specification-sensitivity than polynomial regressions. Accordingly, the local linear regression of equation (1) is my preferred specification throughout.

level, or at both the state and year levels using the dual clustering technique described in Petersen, 2009.)

The RDD is based on inference at the discontinuity threshold, which in this case is where elections are decided by a single vote. Naturally, power considerations make it necessary to expand the sample to include elections with a larger winning margin, but the larger the sample, the more inference is based on observations farther away from the winning threshold and the greater the potential bias, as is true of any RDD setting. Thus, I need to use the smallest winning margin (the "bandwidth" in the RDD terminology) that still provides sufficient power for inference; my relatively large sample - which includes all election cycles from 1979 to 2010 - allows us to use a tight 1% bandwidth. In robustness checks, I show that the results also hold for alternative choices of the bandwidth, and if I use Imbens and Kalyanaraman's (2012) algorithm for optimal bandwidth selection (see Section 3.2 for details).

Finally, I estimate the local linear regression (1) using a triangular kernel. This kernel has been shown to be optimal in estimating local linear regressions at boundaries (Fan and Gijbels, 1996). The triangular kernel weighs observations nearer the threshold more than those farther away—intuitively, the triangular kernel gives more weight to elections whose outcome is more likely to be random. In robustness checks, I show that the results are not sensitive to the choice of the kernel. In particular, I show that my results are unchanged if instead I use a rectangular kernel (i.e., equal weights).

2.3 Tests for Quasi-Random Assignment

My identification strategy relies on the assumption of random assignment to the "treatment" (i.e., being connected to a winning politician). As mentioned in Section 2.2, this assumption has testable implications, akin to the tests of effective randomization in experimental data.

The first testable implication is that companies donating to local candidates who win or lose by a margin of less than 1% of votes should be similar on the basis of *ex ante* characteristics. If they differ, then the treatment is not randomized; instead we would infer that companies are able to predict the election outcome and sort themselves accordingly.

Intuitively, it seems plausible that the outcome of elections that are decided by as little as 1% or less of the votes is close to "pure luck," and hence orthogonal to firm characteristics.

In **Table 2**, I examine whether companies on both sides of the winning threshold are similar on the basis of a large set of characteristics measured in the year preceding the election. These characteristics include Tobin's Q, return on assets (ROA), leverage, cash, size, donation amount, lobbying expenses, stock return (1, 3, and 6 months prior to the election), as well as the beta of the stock.[12] As is shown, companies supporting winners and those supporting losers are very similar along all these characteristics.[13] The difference-in-means test provided in the last column of the table further confirms that the average difference in each characteristic is always insignificant. In untabulated regressions, I further estimate the baseline RDD regression with each covariate as a separate dependent variable. The coefficient on the winner dummy is always small and insignificant, confirming that winner and loser firms are indeed very similar.

A second testable implication of effective randomization is that the number of companies supporting winners and losers should be uniformly distributed on both sides of the discontinuity threshold. In my sample of 604 firms, the number of companies donating to winners and losers is indeed almost identical: 50.2% of the firms donate to future winners (305 firms donate to winners, 299 firms donate to losers). The null of uniform distribution cannot be rejected (p=0.839).[14] Overall, the evidence from Table 2 strongly supports a valid regression discontinuity design: winners and losers are randomly assigned.[15]

In contrast, extending my sample to elections in which the winning margin is higher than 1% would yield unbalanced covariates around the discontinuity threshold, as well as an imbalance in the number of firms supporting winners and losers. In particular, in the sample of elections with a 2% winning margin, 52.1% of the firms donate to future winners (p=0.015); in the sample of elections with a 3% winning margin, 56.6% donate

[12] ROA is the ratio of operating income before depreciation to the book value of assets. Leverage is the sum of long-term debt and debt in current liabilities divided by the book value of assets. Cash is the ratio of cash and short-term investments to the book value of assets. Size is the natural logarithm of the book value of assets. ROA, leverage, cash, and size are obtained from Compustat. Stock return data are obtained from the Center for Research in Security Prices (CRSP). Beta is computed by estimating the market model using daily stock returns in the year preceding the close election. The market portfolio used to estimate the market model is the CRSP value-weighted index consisting of all NYSE, AMEX, and NASDAQ stocks.

[13] Note that the average beta is less than one in both groups. By construction, the sample includes only companies that make political donations. The cash flows of such companies are more likely to be government-dependent and hence less cyclical compared to the average U.S. public company.

[14] The McCrary (2008) test for manipulation of the running variable further supports randomization of the treatment. I are unable to reject the null of continuity of the density function at the threshold ($p = 0.952$).

[15] A third testable implication of randomized assignment to the treatment is that the RDD estimate, i.e. the estimate of β in regression (1), should not vary greatly when I include baseline covariates, as these are not required for consistent estimation of the treatment effect. As I show in Section 3.1.B, this is indeed the case.

to future winners (p=0.000). The fact that more companies donate to future winners is symptomatic of companies anticipating the outcome of the election and betting on the likely winner. Importantly, this pattern suggests that the outcome of "close" elections in which the winning margin is higher than 1% is unlikely to be sufficiently random for a valid RDD. This motivates the choice of a 1% winning margin in my baseline analysis.[16]

3 Results

3.1 Main Results

A. Graphical Analysis I begin the description of the results with a simple graphical analysis. **Figure 1** provides an overall picture of the data. Specifically, the figure plots Δ Tobin's Q of companies donating to local candidates against the candidate's vote share above and below the winning threshold in the larger sample of elections that were decided by less than 3% of votes. The vertical line marks the winning threshold. Companies donating to candidates to the right of the vertical line contribute to winning candidates, companies donating to candidates to the left to losing candidates. Each dot in the figure is an average of Δ Tobin's Q in 0.2% bins of vote share. The solid line plots predicted values of Δ Tobin's Q from third-order polynomials in vote share estimated separately to the left and right of the winning threshold. The dashed lines represent one-standard deviation bounds.

As can be seen in the figure, Δ Tobin's Q appears to be a continuous and smooth function of the candidate's vote share everywhere except at the winning threshold, where there is a large discontinuous jump.[17] This evidence suggests that a close (random) victory of the company's local politician leads to a sharp increase in firm value. Interestingly, Δ Tobin's Q seems to converge to zero as we move further to the left or right of the discontinuity. This pattern suggests that the outcome of elections whose winning margin is higher than a few percentage points is anticipated by the market, and hence any value implication is already impounded in company valuations. This evidence is consistent with Snyder et al. (2011) who show that there is substantial predictability in the outcome of elections whose winning margin is 2% or higher (see also the discussion in Section 2.3).

[16] A similar argument has been made in the political science literature (e.g., Caughey and Sekhon, 2011; Grimmer et al., 2011; Snyder et al., 2011). For example, Snyder et al. (2011) study a large sample of elections to U.S. offices from 1880 to 2009 and find substantial predictability in the outcome of elections that are decided by a winning margin of 2% or higher.

[17] This discontinuity, i.e. the difference between the two polynomial plots at the winning threshold, is highly significant ($p = 0.008$). Higher-order polynomial specifications provide a similar fit.

B. Regression Analysis The graphical analysis in Figure 1 suggests that companies whose local politician is elected by a small margin of votes experience a sharp increase in firm value. To formally test this hypothesis, I estimate several variations of regression (1). The discontinuity at the victory threshold is captured by the winner dummy, which equals one for winners (i.e., companies whose local candidate is elected in a close election) and zero for losers. As discussed in the methodology section, the main specification includes close elections in which the winning margin is less than 1% of total votes. (I consider alternative winning margins in Section 3.2.G.)

The results are presented in **Table 3**. All regressions include year fixed effects and are estimated using a triangular kernel. In column [1], the coefficient on the winner dummy is 0.049 and is statically significant. It is also economically significant. Given that the sample mean of Tobin's Q is 1.4, an increase of 0.049 implies that Tobin's Q increases by 3.5%.

In column [2], I include vote and vote×winner on the right-hand side of the regression. This specification accounts for any linear trend in Δ Tobin's Q on either side of the winning threshold (see Section 2.2). As is shown, the coefficient on the winner dummy is 0.067, which implies an increase in firm value of 4.8%. Albeit larger, this coefficient is comparable to that in column [1].

In column [3], I further include firm-level characteristics as controls in the regression. Specifically, I control for Tobin's Q, ROA, leverage, cash, and size, all measured in the year preceding the election. If the election outcome is truly random, including these covariates should not affect the coefficient on the winner dummy—as in randomized experiments—since all predetermined characteristics should be orthogonal to the assignment of winners versus losers. The coefficient is almost identical to the baseline coefficient in column [2]. This finding is not surprising given the evidence in Table 2 showing that winner and loser firms are very similar on the basis of observable characteristics.[18] Since including firm-level covariates is immaterial for the results, the specification in column [2] is my preferred specification which I use throughout the analysis.

In the analysis so far I have winsorized Δ Tobin's Q at the 1st and 99th percentiles of its empirical distribution to mitigate the effect of outliers. An alternative approach is to estimate median (least absolute deviation) regressions. In columns [4]-[6], I re-estimate

[18] The coefficient on the winner dummy is virtually identical if I control for the full set of characteristics listed in Table 2.

the specifications in columns [1]-[3] using median regressions.[19] As is shown, the coefficient on the winner dummy ranges from 0.032 to 0.038, corresponding to an increase in Tobin's Q by 2.3% to 2.7%. While the increase in firm value is somewhat smaller than before, it remains significant at the 5% level.

Overall, the evidence from Table 3 suggests that a close victory of the company's local politician leads to an increase in firm value by 2.3% to 4.8%. This evidence is consistent with the view that companies benefit from political connections. In the following, I examine the robustness of this finding and study the channels through which political connections increase firm value.

3.2 Robustness

This section presents various robustness checks. Unless otherwise specified, the underlying specification is the one used in column [2] of Table 3.

A. Rectangular Kernel In the baseline specification, I use a triangular kernel. The triangular kernel assigns linearly declining weights on observations as I move farther away from the winning threshold. Intuitively, the triangular kernel puts more weight on elections whose outcome is more likely to be random. In column [1] of **Table 4**, I use a rectangular kernel that assigns equal weight to each observation (see Imbens and Lemieux, 2008). The coefficient on the winner dummy changes very little, suggesting that the results are not sensitive to the choice of the kernel.

B. Third-Order Polynomial of Running Variable In column [2] of Table 4, I use a third-order polynomial function of the running variable (instead of a local linear regression as in the baseline specification) to capture any higher-order trend in Δ Tobin's Q on either

[19] A main issue that arises with median regressions is the computation of standard errors. In the presence of cross-sectional dependence, the asymptotic covariance matrix of Koenker and Bassett (1978), which assumes independent observations, cannot be used. The standard bootstrap approach cannot be used either, as it only corrects for heteroscedasticity. To circumvent this problem, I use a modified bootstrap approach: block bootstrapping. The difference to standard bootstrapping is that instead of drawing single observations, I draw entire blocks of observations. The idea, which is similar to clustering, is to preserve the existing correlation structure within each block while using the independence across blocks to consistently estimate standard errors. Analogously to my clustering approach, I construct blocks at the state level, leaving us with 32 blocks. More precisely, I construct 200 bootstrap samples by drawing with replacement 32 states from the sample. For each bootstrap sample, I estimate the main specification using a median regression and store the coefficients. Standard errors are then calculated based on the empirical distribution of these 200 sets of coefficients.

side of the winning threshold. To reliably estimate the parameters of the polynomial, I follow common practice in the RDD literature and extend the bandwidth of the running variable (e.g., Lee et al., 2004). Specifically, I consider elections where the winning margin is up to 3%. As can be seen, I find that the coefficient on the winner dummy is similar to what I obtained in the baseline specification.[20] I also obtain similar results if instead of a third-order polynomial I estimate a fourth-, fifth-, or sixth-order polynomial.

C. Optimal Bandwidth of Imbens and Kalyanaraman (2012) In column [3] of Table 4, instead of using a bandwidth of 1% of votes, I use Imbens and Kalyanaraman's (2012) optimal bandwidth selection algorithm, a data-dependent method for choosing the bandwidth that is asymptotically optimal. The resulting optimal bandwidth is 1.37%. When I re-estimate the baseline specification using this bandwidth, I find that the coefficient on the winner dummy is very similar to before.

D. Multiple Donations About 10% of the observations are firms donating to more than one local candidate in a given election cycle. For example, it could be that a company donates to two local candidates, one running for Senate, one running for the House of Representatives. Or it could be that the company donates to two local candidates running in the same election. As described in Section 2.1, in these cases, I code the company as supporting the candidate to which it makes the largest donation. Arguably, this coding is subject to measurement error, especially for donation amounts that are of comparable size. To examine whether the coding of multiple donations affects the results, I re-estimate the baseline specification excluding companies making multiple donations. As is shown in column [4] of Table 4, the coefficient on the winner dummy is very similar to before.

E. Unobserved Heterogeneity at the Industry and Election Level The key identifying assumption of the RDD estimation is that companies donating to local candidates who win or lose by a margin of less than 1% of votes are similar ex ante. This assumption is supported by the evidence in Table 2 showing that winner and loser firms are very similar on the basis of several firm-level characteristics. As a corollary, controlling for such characteristics does not affect the coefficient of the winner dummy (see Section 3.1).

[20] This analysis is the regression equivalent of the graphical analysis in Figure 1, which plots predicted values of Δ Tobin's Q from a third-order polynomial in vote share to the left and right of the winning threshold based on a 3% bandwidth (see Section 3.1.A).

Similarly, if the election outcome is truly random, unobserved heterogeneity at the industry and election level should not affect the coefficient on the winner dummy either. To examine whether this is the case, I re-estimate the baseline specification by including industry and election fixed effects. Industries are partitioned using 2-digit SIC (Standard Industrial Classification) codes. The results are presented in columns [5] and [6], respectively, of Table 4. In both specifications the coefficient on the winner dummy is very similar to the baseline coefficient in column [2] of Table 3.[21]

F. Short-Term Stock Market Reaction The dependent variable used in the baseline specification, Δ Tobin's Q, measures the increase in firm value in the two years following the close election (compared to the two years preceding the election). Arguably, if a "randomized" increase in political connections increases firm value, one may expect the stock price to react positively around the announcement of the election results.

To examine the short-term stock market reaction, I conduct an event study around the date of the elections (day 0) and compute the cumulative abnormal return (CAR) in the three-day event window $(-1, +1)$. Abnormal returns are computed using the market model. The market portfolio is the CRSP value-weighted index consisting of all NYSE, AMEX, and NASDAQ stocks. The parameters of the market model are estimated by OLS using 200 trading days prior to the start of the event window. I then use CAR$(-1, +1)$ as dependent variable in the baseline regression. The results are presented in column [7] of Table 4. The coefficient on the winner dummy is equal to 0.011 and is highly significant. This implies that the stock price increases by 1.1% in the three-day event window. This evidence suggests that the stock market does indeed realize that political connections are value-increasing.[22]

[21] In the specification with election fixed effects, the weaker significance of the winner dummy reflects the low power of the test. Since the sample consists of 604 firm-election observations and 101 elections, I have on average only 6 observations per election. Nevertheless, despite the low power of this test, it is reassuring to see that the point estimate of the winner dummy is unaffected.

[22] My CAR result is related to two recent working papers (Do et al., 2012; Akey, 2013) that conduct event studies based on close elections and obtain mixed results. The setting in these papers differs substantially from ours, though. For example, none of these papers considers contributions to local candidates; Do et al. proxy for political connections using data on social networks instead of PAC contributions; Akey examines special (i.e., off-cycle) congressional elections. Importantly, due to the short sample period considered in these studies, the authors have to rely on a winning margin of 5% in order to obtain a sufficient number of "close" elections (13 special elections from 1997-2010 in Akey, 128 congressional elections from 2000-2008 in Do et al.). However, as discussed in Section 2.3 and in the political science literature (e.g., Snyder et al., 2011), there is substantial predictability in the outcome of elections whose voting margin is 2% or higher. In contrast, the comprehensive dataset used in this paper, which covers all election cycles from 1979-2010, allows us to consider a tight 1% winning margin, which likely warrants a causal interpretation of the RDD estimates.

G. Alternative Winning Margins In **Table 5**, I examine the sensitivity of the results to alternative winning margins, bearing in mind the caveat that the outcome of elections whose winning margins is 2% or higher is unlikely to be random (see Section 2.3). Specifically, I consider alternative winning margins ranging from 0.5% (column [1]) to 3.0% (column [6]) in 0.5% increments. (Naturally, the specification in column [2] reproduces the baseline result from Table 3.) As is shown in column [1], the main result holds even if I consider a winning margin as tight as 0.5%. The downside of this specification is that the number of observations drops dramatically (from 604 with a 1% margin to 315 with a 0.5% margin), which reduces the power of the test. This explains the weak significance of the winner dummy despite the point estimate being very similar to before.

For winning margins higher than 1% (columns [3]-[6]), I find that the coefficient on the winner dummy is remarkably stable. However, a look at the number of companies supporting winners and losers (tabulated in the two rows before last) reveals an imbalance between the two. As discussed in Section 2.3, this imbalance suggests that the election outcome is not random. Accordingly, the estimates from columns [3]-[6], albeit consistent with the baseline result, may not warrant a causal interpretation.

3.3 Cross-Sectional Heterogeneity

The value of political connections likely differs across industries. In particular, the payoff may be higher in industries whose revenues depend heavily on the government (e.g., the defense industry). To examine this hypothesis, I classify industries according to their share of procurement contracts (the procurement data are described in Section 4). Specifically, I code as "high government share industries" those 2-digit SIC industries in which the total dollar amount of procurement contracts is above the median across all industries in the year preceding the close election.[23] "Low government share industries" are defined similarly. I then interact the winner dummy with two dummy variables indicating whether the company operates in an industry with high or low government share, respectively. The results are presented i column [1] of **Table 6**. As is shown, the increase in Tobin's Q in high government share industries is larger than the baseline estimate and is highly significant. In contrast, the effect is insignificant in industries with low government share.

Similarly, the value of political connections may be higher in industries where regulations are particularly comprehensive, which I refer to as "regulated industries" (e.g., banking,

[23] I obtain very similar results if I scale the total dollar amount of procurement contracts by total industry sales.

telecommunications). The list of regulated industries is obtained from Coates (2012, p. 675). As can be seen in column [2], I find that indeed the increase in Tobin's Q is larger in regulated industries. Overall, the results from columns [1] and [2] indicate that the value gains from political connections are especially important in industries where business strategy and revenues are directly linked to political decisions.

In column [3], I further examine whether the increase in firm value is stronger for companies whose contribution amount is above the median across all corporate donations in the same election cycle. Arguably, a larger investment in political capital (which may reflect a deeper underlying relationship between company and politician) may translate into a higher payoff. My results are consistent with this hypothesis. The increase in Tobin's Q is large and highly significant for above-median campaign donations, while it is smaller for below-median donations.

Finally, in column [4] I examine whether the increase in firm value differs depending on the party affiliation of the winning candidate. I find that the increase in Tobin's Q is larger for companies donating to a winning Republican as opposed to a winning Democrat. One potential explanation could be the closer ties of Republican politicians to the U.S. corporate sector.[24] Moreover, Republican politicians are known to be more closely connected to firms in the "sin" industries such as tobacco, alcohol, and gambling, whose operations are in general against social norms, and thus are more sensitive to litigation and regulatory risks (Hong and Kostovetsky, 2012). As was shown in column [2], companies operating in industries where regulations are particularly comprehensive experience a larger increase in Tobin's Q following a close victory of their candidate.

3.4 Location and Industry Considerations

A. Donations to Non-Local Candidates In the baseline analysis, I only consider donations to "local" politicians, i.e. politicians whose congressional representation corresponds to the state in which the company conducts most of its operations. As discussed in Section 2.1, local politicians have stronger incentives to bring the political rewards back to the donor companies. In contrast, it seems less likely that a company operating in, say, California would benefit from contributing to a politician representing New Hampshire.[25]

[24] Consistent with this explanation, Cooper et al. (2010) and Goldman et al. (2009) document that the amount of corporate donations to the Republican party is larger. They also find that a greater number of companies make PAC donations to Republican candidates.

[25] There are many reasons why companies may donate to "non-local" politicians. One reason could be ideology (e.g., a company supports all Republican candidates in the U.S.). Another reason could be

To examine whether donations to out-of-state candidates affect firm value, I repeat the analysis, but instead of considering donations to local candidates, I only consider donations to non-local candidates. The results are presented in column [1] of **Table 7**. As is shown, the coefficient on the winner dummy, albeit positive, is small and insignificant. This result suggests that local political connections are beneficial for companies, while non-local connections are not, or only to a marginal extent.

B. Industry Spillovers The regression discontinuity framework ensures that companies are randomly assigned to winners and losers. That being said, *which candidate* they choose to support is not randomized. Accordingly, another potential interpretation of the results—besides companies benefiting from investing in political capital (i.e., donating to candidates)—is that companies choose to support candidates that are ideologically well-disposed to their activities, and thus any ex post benefits the company obtains would have occurred even in the absence of the company's donation.

One way to distinguish between these two interpretations is to examine firms that do not make political campaign contributions and yet are in the same state and 2-digit SIC industry as firms contributing to winning or losing candidates. If what is driving the results is alignment between candidates and specific industries, I should find an "effect" at these companies as well. This analysis is provided in column [2] of Table 7. As can be seen, the coefficient on the winner dummy is small and insignificant. This finding supports my interpretation that being connected to a winner has a direct payoff to the donors.

4 How Do Political Contributions Affect Firm Value?

In this section I examine the channel through which political connections increase firm value. In Section 3.3, I noted that the increase in firm value is higher for companies in industries with high government share. Accordingly, one potential channel through which companies may benefit from political connections is via the allocation of procurement contracts. Arguably, if the elected politician has some influence over the allocation of government contracts, she may favor the company that supported her.

that the company has peripheral operations in the "non-local state". In this case, the motivation for contributing to local and non-local candidates is essentially the same. However, to the extent that the company has only peripheral operations in the non-local state, it is unclear whether the benefits would be sufficiently large to significantly affect firm value.

To examine this hypothesis, I collect information on the amount of government procurement contracts allocated to each firm from the Federal Government Procurement Data System. Specifically, I obtain the full list of government contractors with the outstanding dollar amount of allocated contracts in each month during the sample period. I match contractors to Compustat by company names and aggregate the amount of government procurement contracts in a given calendar year for each firm. I then compute Δ Procurements, which is the change in the ratio of the total allocated government procurement contracts to the book value of total assets two years before and after the close election.

In column [1] of **Table 8** I re-estimate the baseline specification using Δ Procurements as dependent variable. As is shown, the coefficient on the winner dummy is positive, but insignificant. In column [2], where I interact the winner dummy with the indicators for industries with high and low government share, I find a large and significant increase in procurement contracts for firms in the high government share industries. The coefficient of 0.016 implies that the dollar amount of procurement contracts allocated to the winner firm increases by 1.6% of the firm's assets. This result, along with the previous finding that the increase in Tobin's Q is higher in industries with high government share, indicates that government expenditures are a potentially important channel through which companies benefit from political connections.

A second potential channel is mitigation of legislative and regulatory risks. This channel was suggested in Section 3.3, where I noted that the increase in firm value is larger in industries where regulations are particularly comprehensive. To further examine this channel, I construct two measures of risk at the firm level. The first measure is cash flow volatility, which is computed as the standard deviation of earnings per share (EPS) using quarterly Compustat data. The second measure is return volatility, which is the standard deviation of the company's stock return using monthly returns from CRSP. Both measures have the caveat that they do not directly capture legislative and regulatory risks. However, to the extent that such risks affect the companies' operating environment, they may translate into higher earnings and stock market risk. For both measures, I compute the percentage change two years after relative to two years before the election (denoted by Δ Cash flow volatility and Δ Return volatility, respectively). The results based on these two measures are presented in columns [3]-[6] of Table 8. As can be seen from columns [3] and [5], the coefficient on the winner dummy is negative, but only marginally significant. In columns [4] and [6], I decompose the effect depending on whether the company operates in an industry where regulations are particularly comprehensive. I find that the decrease in risk is larger and significant in these industries. This finding is consistent with the view that political connections help mitigate legislative and regulatory risks.

A third channel through which winning firms may benefit from political connections is via improved loan terms, perhaps due to the reduced regulatory and legislative risks discussed above. To examine this hypothesis, I use pricing information on syndicated loans compiled by Dealscan. I construct the relative change in average all-in-drawn spread charged on the company's loan facilities two years before and after the election. The results are presented in columns [7] and [8]. As is shown, the average effect across all firms is negative, but insignificant. When I look at companies in regulated industries—where political connections are more likely to reduce legislative and regulatory risks—I find a larger reduction in the average spread. Specifically, the spread is reduced by 8.5% and is significant at the 10% level (since the average spread is 189 basis points, this corresponds to a spread reduction of 16 basis points).

Finally, a fourth channel could be corporate taxes. Arguably, elected politicians may "repay" companies by helping them obtain a more favorable tax treatment. I find no evidence supporting this channel. Specifically, I re-estimate the regressions using the change in the company's implied tax rate, defined as the average of the ratio of income taxes to pretax income (from Compustat) in the two years after the election minus the average in the two years preceding the election. As is shown in columns [9]-[11] of Table 8, the coefficient on the winner dummy is always small and insignificant, even in industries with high government share and in regulated industries.

5 Lobbying

PAC contributions are relatively small. In the sample, the average contribution is $2,000 in nominal dollars ($3,000 in 2010 dollars). Such amount is in line with U.S. campaign finance law that restricts the amount that can be raised through PAC contributions (see Section 2.1).

Given the relatively small size of individual PAC donations, it seems puzzling that contributing to a winning candidate brings about substantial value gains. One potential explanation is that campaign contributions open doors to a more active political engagement of the donor firms after the election. In particular, companies whose "political ally" is elected may have stronger incentives to engage in lobbying, even if doing so is costly.[26] This hypothesis has been suggested in the political science literature where it is known

[26] Several recent studies document that corporate political lobbying is associated with a reduction in the firm's litigation and regulatory risks (e.g., Chen et al. 2010; Yu and Yu, 2011). See Bertrand et al. (2012) for an overview of the empirical literature on lobbying.

as the "access view" of campaign contributions (e.g., Ansolabehere et al., 2002). In this view, campaign contributions provide access to legislators and hence an entry point for subsequent lobbying efforts.

To obtain information on corporate lobbying expenditures, I manually collect data from the Lobbying Disclosure Electronic Filing System of the Senate's Office of Public Records. This database starts in 1998 and contains information on lobbying expenditures for each client firm and each lobbyist. I aggregate the lobbying expenditures for the firms in the sample on an annual basis.[27] Unlike PAC contributions, lobbying expenditures are fairly substantial. As is shown in Table 2, average annual lobbying expenditures are over $0.5 million.[28]

To examine whether companies increase their lobbying efforts after their local candidate is elected by a narrow margin of votes, I compute the change in the ratio of total annual lobbying expenses to the book value of total assets two years after minus two years before the election, denoted by Δ Lobbying expenses. I then use Δ Lobbying expenses as dependent variable in the RDD regression. The results are presented in **Table 9**. As is shown in column [1], the coefficient on the winner dummy equals 0.007, which implies that lobbying expenses of winner firms increase by 0.7% of the firm's assets. This coefficient is insignificant, though.[29] In columns [2] and [3], I decompose the effect depending on whether the company operates in an industry with high government share and a regulated industry, respectively. As can be seen, the increase in lobbying expenses is larger and significant in these industries. This evidence indicates that firms increase their lobbying efforts following the election of their local candidate, and this is especially the case for companies in government-dependent industries. Overall, these findings suggest that PAC contributions are part of a more complex relationship between firms and elected politicians—seemingly small campaign contributions may reflect the tip of the iceberg of a more substantial political engagement of companies.

6 Conclusion

This paper provides evidence of a causal link between companies' political campaign contributions and subsequent benefits to the companies involved. I use a RDD approach that

[27] Lobbying expenditures are disclosed semi-annually until 2007, and afterward on a quarterly basis.

[28] Similarly, Bombardini and Trebbi (2012) report that total lobbying expenditures were $2.6 billion, versus $345 million in congressional campaign contributions over the 2005-2006 election cycle.

[29] The weak significance of the results in Table 9 may reflect the small sample size (146 observations). Since the lobbying data start in 1998 I lose more than two thirds of the initial sample.

focuses on U.S. congressional elections from 1980 to 2010 that were decided by less than 1% of votes. Such close call elections are akin to a randomized assignment of companies to winning and losing candidates and therefore provide exogenous variation in political connections.

I find that contributing to a winning candidate leads to an increase in firm value (as measured by Tobin's Q) by 2.3% to 4.8% relative to contributing to a losing candidate. This effect is only found for companies supporting local candidates, not for out-of-state candidates, which highlights the importance of local political connections. The increase in firm value is robust to a wide range of alternative specifications of the RDD, such as the use of polynomials of the running variable, different kernels for the local linear regression, including a variety of covariates and fixed effects, using alternative winning margins, minimizing mean absolute deviations, and excluding companies that contribute to multiple candidates. Finally, the results also hold if I examine the short-term stock market reaction around the announcement of the election outcome instead of studying long-term changes in firm value. Specifically, I find that the difference in the three-day cumulative abnormal return of companies supporting winners compared to companies supporting losers is 1.1%. When I examine the heterogeneity in the treatment effect, I find that the increase in firm value is highest for companies operating in government-dependent industries and for companies whose contribution is greater than the median amount. Overall, my findings indicate that companies obtain material benefits from local political connections, consistent with the notion that corporate campaign contributions represent a valuable investment for companies rather than a waste of resources that would be symptomatic of an agency problem.

I further document evidence supporting three channels through which political connections increase firm value: 1) allocation of procurement contracts, 2) mitigation of legislative and regulatory risks, and 3) improved access to bank financing. In contrast, I find no evidence that companies use their political connections to achieve a more favorable tax treatment. Finally, I find that companies supporting winners are more likely to increase their lobbying effort following the election. This finding suggests that companies whose political ally is elected have stronger incentives to take further action to induce favorable treatment. This also suggests that the benefits obtained by companies stem from a deeper and more complex relationship than a simple transactional relationship between companies and politicians. Understanding the various facets of this relationship is an exciting avenue for future research.

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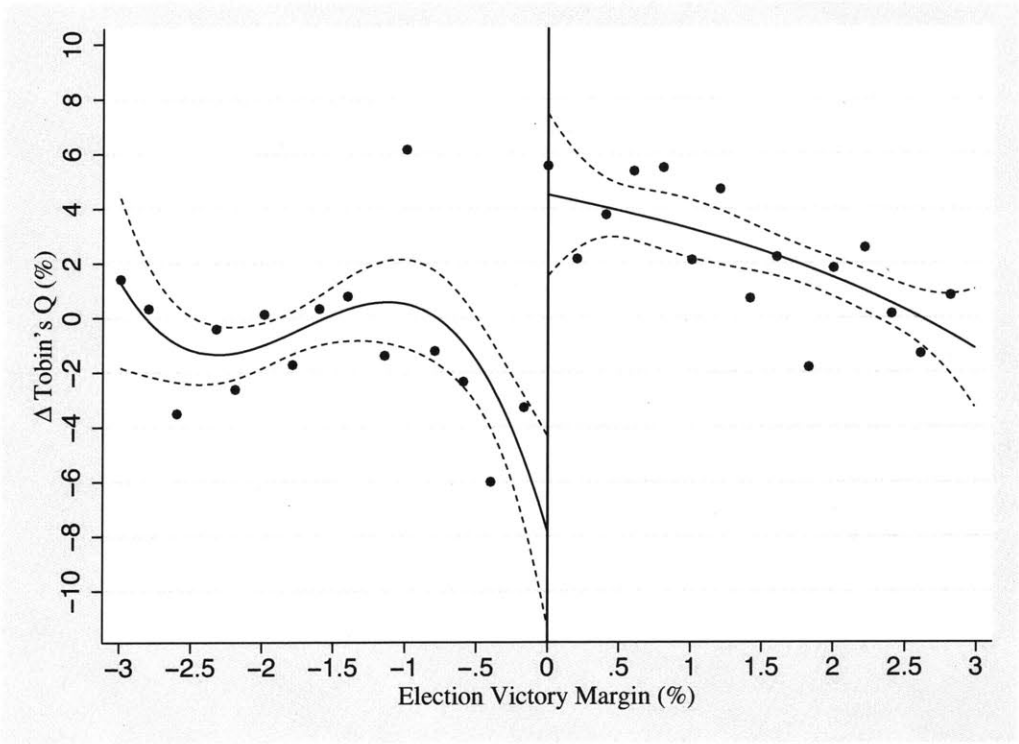
8 Figures and Tables

Figure 1: Political Campaign Contributions and Firm Value



The horizontal axis indicates the candidate's vote share above and below the winning threshold ("winning margin"). The vertical line marks the winning threshold. The vertical axis indicates Δ Tobin's Q, which is the average Tobin's Q in the two years after the election minus the average Tobin's Q in the two years preceding the election. Tobin's Q is the market value of assets divided by the book value of assets, where the market value of assets is the book value of assets plus the market value of common stock minus the sum of the book value of common stock and balance sheet deferred taxes. Dots are average values of Δ Tobin's Q in 0.2% bins of vote share. The solid line plots predicted values of Δ Tobin's Q from third-order polynomial OLS regressions of Δ Tobin's Q on the candidate's vote share to the left and right of the winning threshold. Dashed lines represent one-standard deviation bounds. The sample includes all elections from 1980 to 2010 that are decided by less than 3% of votes.

Table 1: Election Statistics

Panel (A): Election Outcomes

	N	Mean	Std. Dev.	25th	Median	75th
Winning Margin (% of votes)	101	0.48	0.29	0.25	0.51	0.72
Votes of Winner (% of votes)	101	49.2	1.5	48.7	49.8	50.2
Votes of Runner-up (% of votes)	101	48.7	1.5	48.3	49.2	49.7

Panel (B): Election Types

	N	%
House Elections	88	87.1
Senate Elections	13	12.9
Elections Won by Democrats	51	50.5
Elections Won by Republicans	50	49.5

Panel (A) provides summary statistics for election outcomes. Winning margin is the difference in the percentage of votes obtained by the winner and the runner-up. Votes of winner (runner-up) is the percentage of votes obtained by the winning (runner-up) candidate. Panel (B) provides a breakdown of the close elections according to the type of election (House of Representatives or Senate) and the winner's party (Republican or Democrat). The sample contains 101 close elections from 1980 to 2010.

Table 2: Summary Statistics

	Winners (N = 305)					Losers (N = 299)					p-value (Difference
	Mean	Std. Dev.	25th	Median	75th	Mean	Std. Dev.	25th	Median	75th	in Means)
Tobin's Q	1.384	0.687	1.008	1.111	1.493	1.392	0.739	0.994	1.109	1.513	0.903
ROA	0.126	0.077	0.067	0.125	0.175	0.124	0.081	0.060	0.125	0.176	0.716
Leverage	0.255	0.164	0.136	0.232	0.344	0.229	0.139	0.127	0.226	0.319	0.242
Cash	0.071	0.089	0.014	0.043	0.094	0.078	0.090	0.018	0.044	0.104	0.366
Size (logarithm)	8.489	1.691	7.392	8.433	9.643	8.578	1.839	7.322	8.415	9.951	0.480
Donation Amount ($)	2,134	2,189	500	1,000	3,000	2,014	2,033	500	1,000	2,850	0.610
Lobbying Expenses ($1,000)	527	120	80	225	544	564	972	89	295	544	0.837
Stock Return (1-month)	0.011	0.079	-0.044	0.016	0.060	0.008	0.080	-0.051	0.016	0.056	0.866
Stock Return (3-month)	0.035	0.132	-0.057	0.044	0.113	0.035	0.138	-0.063	0.039	0.122	0.998
Stock Return (6-month)	0.021	0.156	-0.084	0.022	0.127	0.002	0.163	-0.093	0.008	0.117	0.168
Beta	0.902	0.636	0.463	0.960	1.313	0.964	0.621	0.583	0.992	1.379	0.376

"Winners" refers to the sample of companies donating to a local candidate who is elected in a close election (i.e., an election in which the winning margin is less than 1%). "Losers" is defined analogously. Tobin's Q is the market value of assets divided by the book value of assets, where the market value of assets is the book value of assets plus the market value of common stock minus the sum of the book value of common stock and balance sheet deferred taxes. Return on assets (ROA) is the ratio of operating income before depreciation to the book value of assets. Leverage is the sum of long-term debt and debt in current liabilities divided by the book value of assets. Cash is the ratio of cash and short-term investments to the book value of assets. Size is the natural logarithm of the book value of assets. Tobin's Q, ROA, leverage, cash, and size are all measured in the year preceding the close election. Donation amount is the company's PAC contribution to the local candidate running in the close election. Lobbying expenses are the company's total lobbying expenditures in the year preceding the close election. Stock return (1-month) is the company's stock return in the month preceding the close election. Stock return (3-month) and stock return (6-month) are defined similarly. Beta is obtained by estimating the market model using daily stock returns in the year preceding the close election. The sample includes all close elections from 1980 to 2010 (604 firm-year observations), except for lobbying expenses which are only available for the sample of close elections from 2000 to 2010 (146 firm-year observations).

Table 3: Do Political Campaign Donations Increase Firm Value?

Dependent Variable:	Δ Tobin's Q					
	[1]	[2]	[3]	[4]	[5]	[6]
Winner	0.049***	0.067**	0.064**	0.032***	0.038**	0.035**
	(0.019)	(0.031)	(0.030)	(0.011)	(0.016)	(0.014)
Local Linear Trend	No	Yes	Yes	No	Yes	Yes
Control Variables	No	No	Yes	No	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Regression Type	OLS	OLS	OLS	Median	Median	Median
Observations	604	604	604	604	604	604
R-squared	0.18	0.19	0.21	0.09	0.09	0.11

Winner is a dummy variable that equals one if the company donates to a local candidate who is elected in a close election (i.e., an election in which the winning margin is less than 1%) and zero otherwise. Δ Tobin's Q is the average Tobin's Q in the two years after the election minus the average Tobin's Q in the two years preceding the election. Control variables include ROA, leverage, cash, and size, all measured in the year preceding the election. Tobin's Q and all control variables are defined in Table 2. All regressions are estimated using a triangular kernel as described in Section 2.2. In columns [1]-[3], standard errors are clustered at the state level. In columns [4]-[6], median regressions are used, where the standard errors are computed using block bootstrapping with 200 bootstraps and state-level blocks. The sample includes all close elections from 1980 to 2010. Standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels respectively.

Table 4: Robustness

Dependent Variable:	Δ Tobin's Q						CAR(-1, +1)
	Rectangular Kernel	Polynomial in Running Variable (3% Voting Margin)	Imbens and Kalyanaraman's (2012) Bandwidth	No Multiple Donations	Industry Fixed Effects	Election Fixed Effects	Stock Market Reaction
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Winner	0.073**	0.078**	0.060**	0.061**	0.071**	0.065*	0.011***
	(0.030)	(0.036)	(0.028)	(0.030)	(0.031)	(0.039)	(0.003)
Local Linear Trend	Yes	No	Yes	Yes	Yes	Yes	Yes
Local Polynomial Trend	No	Yes	No	No	No	No	No
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	No	No	No	No	Yes	No	No
Election Fixed Effects	No	No	No	No	No	Yes	No
Observations	604	1,904	823	545	604	604	604
R-squared	0.14	0.12	0.16	0.19	0.28	0.36	0.04

This table presents variants of the main regression in column [2] of Table 3. In column [1], the regression is estimated using a rectangular kernel instead of a triangular kernel. In column [2], the regression includes a third-order polynomial of the running variable on both sides of the winning threshold in the larger sample of elections in which the winning margin is less than 3%. In column [3], Imbens and Kalyanaraman's (2012) optimal bandwidth is used. In column [4], companies donating to more than one candidate are excluded. In columns [5], and [6], the regressions include industry and election fixed effects, respectively. Industries are partitioned at the 2-digit SIC level. In column [7], the dependent variable is the cumulative abnormal return (CAR) in the three-day event window (-1, +1) around the announcement of the election outcome (day 0). Abnormal returns are computed using the market model. The coefficients of the market model are estimated by OLS using 200 trading days prior to the start of the event window. Standard errors are clustered at the state level. The sample includes all elections from 1980 to 2010. Standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels respectively.

Table 5: Alternative Bandwidths for the Discontinuity Threshold

Dependent Variable:	Δ Tobin's Q					
Voting Margin:	0.50%	1.00%	1.50%	2.00%	2.50%	3.00%
	[1]	[2]	[3]	[4]	[5]	[6]
Winner	0.071*	0.067**	0.068**	0.064**	0.057**	0.051**
	(0.040)	(0.031)	(0.028)	(0.025)	(0.022)	(0.020)
Local Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	315	604	912	1,274	1,586	1,904
# Winners	158	305	497	664	883	1,077
# Losers	157	299	415	610	703	827
R-squared	0.23	0.19	0.17	0.15	0.14	0.13

This table presents variants of the main regression in column [2] of Table 3 where close elections are defined using alternative winning margins ranging from 0.5% (column [1]) to 3.0% (column [6]) in 0.5% increments. Standard errors are clustered at the state level. The sample includes all close elections from 1980 to 2010. Standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels respectively.

Table 6: Cross-Sectional Heterogeneity

Dependent Variable:	Δ Tobin's Q			
	[1]	[2]	[3]	[4]
Winner × High Government Share	0.092***			
	(0.035)			
Winner × Low Government Share	0.056			
	(0.036)			
Winner × Regulated Industry		0.072**		
		(0.031)		
Winner × Other Industry		0.049		
		(0.043)		
Winner × (Donation Amount ≥ Median)			0.085**	
			(0.037)	
Winner × (Donation Amount < Median)			0.057	
			(0.036)	
Winner × Republican				0.082**
				(0.031)
Winner × Democrat				0.042
				(0.036)
Local Linear Trend	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	604	604	604	604
R-squared	0.20	0.19	0.20	0.20

High Government Share is a dummy variable that equals one if the company operates in a 2-digit SIC industry in which the total dollar amount of procurement contracts is above the median across all industries in the year preceding the close election. Low Government Share is defined similarly. Regulated Industry is a dummy variable that equals one if the company operates in an industry in which regulations are particularly comprehensive. The list of regulated industries is obtained from Coates (2012, p. 675). Other Industry is a dummy variable that equals one for the remaining industries. (Donation Amount ≥ Median) and (Donation Amount < Median) are dummy variables indicating whether the company's donation to the candidate is above or below the median across all corporate donations in the same election cycle. Republican and Democrat are dummy variables that indicate whether the candidate supported by the company is Republican and Democrat, respectively. All other variables are defined in Table 3. All regressions include the variable interacted with Winner as control. Standard errors are clustered at the state level. The sample includes all close elections from 1980 to 2010. Standard errors are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Table 7: Non-Local Candidates and Industry Spillovers

Dependent Variable:	Δ Tobin's Q	
	[1]	[2]
Winner (Non-Local Candidate)	0.011	
	(0.010)	
Winner (Industry)		0.033
		(0.036)
Local Linear Trend	Yes	Yes
Year Fixed Effects	Yes	Yes
Observations	5,779	419
R-squared	0.15	0.09

This table presents variants of the regression in column [2] of Table 3. In column [1], the sample consists of companies that donate to non-local candidates running in close elections. In column [2], the sample consists of companies that do not make political campaign contributions but operate in the same 2-digit SIC industry and are located in the same state as companies that donate to local candidates running in close elections. Winner (Non-Local Candidate) and Winner (Industry) are dummy variables indicating whether the non-local candidate and the candidate supported by a company in the same 2-digit SIC industry and state, respectively, is elected in a close election. Standard errors are clustered at the state level. The sample includes all close elections from 1980 to 2010. Standard errors are in parentheses. *, **, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Table 8: How do Political Contributions Affect Firm Value?

Dependent Variable:	Δ Procurements		Δ Cash Flow Volatility		Δ Return Volatility		Δ Loan Spread		Δ Taxes		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]
Winner	0.008		-0.085		-0.092*		-0.043		-0.005		
	(0.006)		(0.057)		(0.053)		(0.041)		(0.035)		
Winner × High Government Share		0.016**								-0.009	
		(0.006)								(0.045)	
Winner × Low Government Share		0.003								-0.002	
		(0.007)								(0.037)	
Winner × Regulated Industry				-0.118*		-0.124**		-0.085*			-0.016
				(0.069)		(0.058)		(0.047)			(0.041)
Winner × Other Industry				-0.064		-0.060		-0.012			0.009
				(0.062)		(0.063)		(0.046)			(0.033)
Local Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	211	211	604	604	604	604	257	257	604	604	604
R-squared	0.08	0.10	0.11	0.11	0.34	0.34	0.19	0.20	0.03	0.03	0.04

Procurements is the total dollar amount of government procurement contracts divided by the book value of total assets. Δ Procurements is the average of procurements in the two years after the election minus the average in the two years preceding the election. Δ Cash Flow Volatility is the percentage change in cash flow volatility in the two years after the election relative to the two years before the election, where cash flow volatility is the standard deviation of earnings per share (EPS) using quarterly data. Δ Return Volatility is the percentage change in stock return volatility in the two years after the election relative to the two years before the election, where return volatility is the standard deviation of the company's stock returns using monthly returns. Δ Loan Spread is the percentage change in average all-in-drawn spread charged on the company's loan facilities in the two years after the election relative to the two years before the election. Taxes is the ratio of income taxes to pretax income. Δ Taxes is the average of taxes in the two years after the election minus the average in the two years preceding the election. All other variables are defined in Tables 3 and 6. The regressions in columns [2], [4], [6], [8], [10], and [11] include the variable interacted with Winner as control. Standard errors are clustered at the state level. The sample includes all close elections from 1980 to 2010. Standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels respectively.

Table 9: Lobbying Expenses

Dependent Variable:	Δ Lobbying Expenses		
	[1]	[2]	[3]
Winner	0.007		
	(0.006)		
Winner × High Government Share		0.012*	
		(0.007)	
Winner × Low Government Share		0.005	
		(0.006)	
Winner × Regulated Industry			0.011*
			(0.007)
Winner × Other Industry			0.006
			(0.006)
Local Linear Trend	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	146	146	146
R-squared	0.07	0.08	0.08

Lobbying expenses is total lobbying expenditures divided by the book value of total assets. Δ Lobbying expenses is the average of lobbying expenses in the two years after the election minus the average in the two years preceding the election. All other variables are defined in Tables 3 and 6. The regressions in columns [2] and [3] include the variable interacted with Winner as control. Standard errors are clustered at the state level. The sample includes all close elections from 2000 to 2010. Standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels respectively.

Table 10: APPENDIX - Close Elections Across States and Election Cycles

Panel (A): Distribution Across States

State	Frequency	Percent
Alabama	3	2.97
Arizona	1	0.99
California	8	7.92
Colorado	1	0.99
Connecticut	5	4.95
Florida	4	3.96
Georgia	2	1.98
Idaho	1	0.99
Illinois	7	6.93
Indiana	4	3.96
Iowa	1	0.99
Kentucky	5	4.95
Louisiana	2	1.98
Maryland	3	2.97
Michigan	2	1.98
Minnesota	6	5.94
Missouri	1	0.99
Montana	1	0.99
Nebraska	1	0.99
Nevada	3	2.97
New Jersey	2	1.98
New York	4	3.96
North Carolina	8	7.92
Ohio	2	1.98
Oregon	2	1.98
Pennsylvania	8	7.92
Texas	2	1.98
Utah	2	1.98
Virginia	5	4.95
Washington	3	2.97
Wisconsin	1	0.99
Wyoming	1	0.99
Total	101	100

Panel (B): Distribution Across Election Cycles

Year	Frequency	Percent
1980	8	7.92
1982	8	7.92
1984	7	6.93
1986	7	6.93
1988	8	7.92
1990	3	2.97
1992	4	3.96
1994	12	11.88
1996	9	8.91
1998	3	2.97
2000	5	4.95
2002	3	2.97
2004	1	0.99
2006	9	8.91
2008	6	5.94
2010	8	7.92
Total	101	100

The sample includes all close elections from 1980 to 2010.